

OBITUARY NOTICES

GEORGE FREDERICK STOUT, 1860-1944

For future historians of psychology it well may be that the death of Stout will mark the end of an era, an age in which the course taken by the philosophy of nature was determined by developments in the philosophy of mind. Recent trends in philosophical psychology may be no less significant than those we owe to Stout, but they belong in the main to the history of philosophy. Stout was first and foremost a psychologist, albeit one who was so at home in the fields of philosophy that he could pass from one domain to the other without any sense of trespass. He belonged to the tradition of the British empiricists. Of this tradition he was both a sympathetic and a radical critic; and he enriched it by assimilating all that was most vital in the contrasting streams of continental thought. It was characteristic of his genius for synthesis that he should acknowledge indebtedness equally to Spinoza and to Hume.

The story of his life could be quickly told. It would be a simple tale of a quiet academic life in college courts and in the countryside, an account of books and lectures, of long walks and lively conversations, of uncomplicated friendships and a happy marriage. It would be a tale devoid of incident, but rich in anecdote, richer still in the memories it leaves with those who enjoyed the friendship, the wit, the wisdom and the kindness of a very great but unassuming man.

These memories will perish. What remains are the fruits of his intellectual life contained in the three great works, the *Analytic Psychology*, the *Manual of Psychology* and *Mind and Matter*, together with a sheaf of papers contributed to leading philosophical and psychological journals of his day.¹ The *Analytic Psychology* was published in 1896, the year in which he left Cambridge to assume the Anderson Lectureship at Aberdeen. There he wrote the *Manual*, and its appearance coincided with his translation to the Wilde Readership at Oxford in 1899. In 1903 he was appointed to the Chair of Logic and Metaphysics at St Andrews and his third great work, *Mind and Matter*, was published in 1931 a few years before his retirement.

For one who did not write with ease his output was prodigious. During his thirteen years at Cambridge, when he held a lectureship in psychology and a Fellowship at St John's, he was one of the most active members of the Aristotelian Society. He was also Editor of *Mind* from 1891 to 1920. Some of his most interesting papers, and some of the most important chapters of the *Analytic Psychology*, first appeared as papers to the Aristotelian Society or as articles in *Mind*. His papers in their totality covered most of the fundamental problems in psychology, metaphysics, epistemology and logic. But through the diversity of these studies there progressively appear the outlines of a coherent and well-knit design. Almost every paper had an intimate bearing upon the systematic philosophy of Mind and Nature that began to assume its final form in the Hifford Lectures which he delivered at Edinburgh in 1919 and 1921. But the first volume in which this system was destined to appear, *Mind and Matter*, was not published until

¹ The most important papers were republished as *Studies in Philosophy and Psychology*. A complete bibliography has been published by J. A. Passmore in the *Australasian Journal of Psychology and Philosophy* (September 1944).

ten years later. The second volume under the projected title of 'God and Nature' was never completed.

His most important and original contributions to psychology had been made, however, in the *Analytic Psychology*, in the *Manual* and in the papers republished in the *Studies of Philosophy and Psychology*. The *Analytic Psychology* immediately secured for him a position of international fame. This book has always been a reference work of psychologists themselves, just as the *Manual* has always been the text-book of students. Those who have views of their own to present, on matters of principle, have always taken the precaution of consulting the *Analytic Psychology* on the related issues. It is in the *Analytic Psychology* that the Gestalt psychologists admit the presence of some measure of anticipation of their findings. Here too, in the doctrine of anoetic sentience Titchener finds his strongest argument against Brentano's doctrine (and, incidentally, Stout's own doctrine) of 'objective reference' as a criterion of mind. So, too, it is in the *Analytic Psychology* that we may still find discussions relevant to recent and current issues. In his doctrine of 'relative suggestion' Stout had turned to psychological account Bradley's principle that 'association marries universals', a principle that has again come to the fore in studies of generalization in conditioning and learning.

The *Manual* is not less original than the *Analytic Psychology*, and its contents have a much more intimate relation to the final philosophy of mind developed in the Gifford Lectures. But to attempt any elucidation of the many and subtle changes in the system of psychology presented through the five editions of this work would call for a more detailed analysis than would be appropriate to this occasion. It must suffice to say a word about Stout's psychology as a whole and to attempt a tentative estimate of his central contributions to the science and philosophy of mind.

The most impressive advances in psychology in the present century have been made by those who have resolutely devoted their attention to strictly empirical issues and with equal resolution refused to be delayed by discussions of philosophical problems. Such psychologists have not unnaturally tended to adopt some simple philosophy of mind and nature, and to make assumptions which they were not prepared explicitly to defend. It is therefore of quite considerable interest to find that the sort of philosophy of mind which so many empiricists pragmatically assume is in so many respects the sort of philosophy for which Stout attempted to supply a rational justification.

It is perhaps not an undue simplification to suggest that the whole troubled history of the theory of body and mind is the story of a great detour—from the essentially biological position of Aristotle through the dualism of Descartes, and back to a biological solution in terms of some such conception as that of Stout's 'embodied mind'. It was, of course, unfortunate that the detailed exposition of Stout's attempted solution was so long delayed. The prevailing tendency throughout the successive editions of the *Manual* was towards a parallelistic position qualified by honest but confusing concessions to the interactionist position. We all read and misunderstood this discussion of the matter presupposing a Cartesian, or perhaps an idealistic, philosophy of mind. It was precisely because in the *Manual* Stout endeavoured to exclude philosophical discussion that his parallelistic conclusion remained, as mere parallelism must inevitably remain, an exasperating mystery. Moreover, in the *Manual* the problem is discussed in abstraction from other questions. But Stout's solution, when it comes, is found to be logically connected with his analysis of perception, more especially with his doctrine of 'internal perception'

briefly outlined in the fourth edition of the *Manual* and occupying a key position in *Mind and Matter*.

The problem to which Stout, in his middle years, devoted most sustained attention was that which is somewhat curiously described—curiously at least to modern ears—as ‘the genetic problem of cognition’, the problem which Ward had said to be of chief interest in psychology, ‘that concerning the formation of complex ideas out of simple ones, of perceptions out of sensations, of thoughts out of perceptions’. Ward had already done much to remove from this discussion the shackling assumptions of associationism. Stout carried further the work that Ward had begun, and carried it to a point at which the results of psychological analysis could be turned to account in epistemological inquiries. He was interested in the processes of mind in relation to their product, the awareness that is gained thereby of ourselves and ‘the external world’. The direction of his interest is already to be discerned in his first important article—the admirable exposition of Herbartian psychology (*Mind*, 1888).

This was the only purely expository work he ever produced. His aim, he said, was ‘to state with the least possible comment the views of Herbart as expounded in his two important works’. In this article Stout restrains, as fortunately he never again restrained, his impulse to criticize, synthesize and develop. But for all his restraint Stout’s interests found their inevitable expression in selection and emphasis. Herbart, he concluded, had rightly regarded his account of ‘Ego-consciousness’ as his crowning achievement. Half a century later Stout might well have said that he regarded his own account of the awareness of the Self as *his* crowning achievement.

The originality of his approach lay in its extreme simplicity. Other psychologists had tried to explain awareness of the external world and of oneself as a complex process of association or creative synthesis or in terms of purely hypothetical processes of integration or eduction, and many philosophers had supposed it to be the outcome of an involved process of deduction. Stout succeeded in showing how much of the knowledge may be arrived at by inductive procedures implicit in common sense, procedures which are of course in operation long before they admit of explicit formulation. At even the simplest level of perceptual experience we are conscious not of the co-existence and sequence of sensations atomistically conceived but of changes which are partially covariant and partly independent. The awareness of any ‘thing’ whatever is the outcome of the correlation of data from more than one sense (and it is clear that the sense of the term ‘correlation’ as here used is essentially the same as that in which it has come to be so important in the statistical techniques applied to the data of mental tests). The distinction between the external world and the self turns out to be precisely the distinction between what is and what is not independent of the process of perception.

The data of the various special senses clearly present, on any view, an incredibly complex tissue of partial correlations which if measured and symbolically expressed as accurately as they are in fact experienced would admit the same procedures of statistical analysis as are commonly applied to any table of co-variations.¹ Stout’s account of the matter is one that inevitably suggests that the status of a ‘physical object’ in the world is in the last analysis precisely the status of any ‘underlying factor’ disclosed by these

¹ It was this complex of covariances that Stout had in mind when he remarked to me that the old-fashioned psychophysical parallelist was preoccupied with one particular case whereas he, Stout, found parallelism almost everywhere.

statistical procedures. It was unfortunate that Stout did not interest himself in these statistical developments to the point of expressing any opinion as to what this status is. One can certainly say, however, what, on his view, that status was *not*. It was not the status of an unknown and unknowable 'thing-in-itself'. Probably no one more than Stout has turned Kantian ideas to good psychological account, but the only use he ever found for any alleged unknowable was as a challenge and incitement to cognition and empirical description. It was on this issue, in respect of the Self, that Stout diverged most sharply from the doctrines of Ward. Beginning as a philosophical idealist, the development of his sympathies, both philosophical and psychological, was progressively in the direction of the realistic, empiricist, naturalistic, perhaps even the phenomenistic and behaviouristic trends of his day.

It is significant that we do not think of Stout, the most original and the most pervasively influential of British psychologists, as the founder of a school. The 'contemporary schools' have in general made their names by the use of a few distinctive concepts and by the illumination of particular fields. Stout, on the other hand, was essentially concerned with a general and systematic theory of mind, and of mind in relation to nature as a whole. The advance for which he was responsible was an advance upon the broadest front, and one in which progress was combined with consolidation. Such an advance could not have the tempo with which in special empirical fields psychologists in recent years have come to be accustomed. But the general theoretical developments to which Stout has substantially contributed may yet be found to contain a philosophical basis for some of the more sensational doctrines which have tickled the curiosity of the interested public.

Stout was not the founder of a school because he was very much more than that. His position was in the middle of the stream, and that the main stream, of psychological thought. It is as tributaries to this stream that the schools find their justification. Stout justifies himself in a converse way. It is in no small measure due to him that the central body of psychological doctrine has assumed a form in which it can assimilate so much. Stout read his contemporaries, as he talked to them, always with the purpose of finding sense and value in what they said, but what he received from them he transformed in the process of ingestion. Others influenced him mainly by stimulating the further elaboration of ideas already in his mind. It was the genius of Stout to assimilate and to synthesize, to graft upon the stock of tradition the sapling shoots from an immense variety of schools.

C. A. MACE

CHARLES EDWARD SPEARMAN, 1863-1945

We record with deep regret the death of Professor C. E. Spearman, formerly Professor of Psychology at University College, London. A full obituary notice will follow in a later number of this *Journal*.

AN ATTEMPT AT AN EXPERIMENTAL APPROACH TO THE PSYCHOLOGY OF RELIGION

BY A. T. WELFORD, *From the Cambridge Psychological Laboratory*

I. *Introduction* (pp. 55-56). II. *Method* (pp. 56-57). III. *Results* (pp. 58-67): (1) *Preliminary description* (pp. 58-60); (2) *The differences between groups having different degrees of churchgoing* (pp. 60-62); (3) *The differences between groups not working with children and the corresponding groups working with children* (pp. 62-63); (4) *The differences between groups of different ages* (pp. 63-64); (5) *Special groups* (pp. 64-65); (6) *The effect of familiarity* (pp. 65-66); (7) *The effect of archaic language* (pp. 66-67). IV. *Summary of conclusions* (pp. 67-68). *Appendix* (pp. 69-73). *References* (p. 73).

I. INTRODUCTION

The psychological aspects of religion have been the subject of a number of investigations during the past few decades, but it is noteworthy that the quantity of work has not been great when the vastness and complexity of the field are considered. To some extent this is probably due to these very features having appalled and confused would-be investigators, but much more it is likely to be due to the fact that such investigations are very difficult to make. Not only is the material often of a vague and intangible nature, but religious people usually resent any attempt to probe into the mysteries of their religious experience and behaviour, feeling them to be something of which the essential nature and charm are likely to be destroyed by analysis. It is doubtless because of this that the method of the majority of investigations has been either that the investigator has looked at those around him and reported and discussed what he has seen, or that he has studied, compared and analysed the material contained in biographies. The shortcomings of both these methods are obvious: the treatment of the material is inevitably subjective, and the material itself not only is limited and somewhat random, but very often fails to give information at just those points where information would seem to be most desirable and illuminating.

A number of workers have overcome these difficulties by the use of questionnaires, which enable wider, closer and more objective surveys to be made. The questionnaire, however, has disadvantages of its own, in particular the danger that, as it is not easy to disguise its purpose, subjects are very likely, whether consciously or unconsciously, to answer in a manner calculated to please (or annoy) the investigator, or to flatter themselves.

These difficulties can be eliminated, or at any rate greatly reduced, by means of carefully designed experiment. Some truly experimental studies of the psychological aspects of religion have been carried out (1), but their number is small, and the techniques still uncertain. The present investigation was undertaken not so much for the sake of any findings which might be obtained, but more as a trial of one possible technique and as an attempt to discover some of the possibilities and difficulties of an experimental approach to the psychology of religion.

The problem chosen was one which seemed to lend itself to such an approach. The language not only of present-day Christianity, but of many—perhaps almost all—religions shows a marked tendency towards the use of archaic forms, even though these may, in

some cases, be almost if not quite unintelligible to worshippers. As the Church in England has often been criticized for this archaism, it was thought to be of interest to try to discover to what extent archaic language really was preferred amongst churchgoers and to what extent such preference was due to familiarity.

It may here at once be said that, as a means of providing illumination on this point, the experiment cannot be regarded as a great success. It does, however, show up a number of other interesting facts a little way removed from the point to which it was originally directed, and can perhaps be said to be relevant to that point in so far as it shows that the question of archaic as opposed to modern language for prayers is very much bound up with a number of other factors, and by itself is probably of little importance. The main bulk of this paper is therefore taken up with the setting out of these other factors which appear to be of interest.

The technique also left much to be desired for, although at the outset it appeared to be one which was likely to be simple to operate, it proved unexpectedly difficult in some respects, and the issues involved in analysing the results were extremely complex. An attempt has been made to extract the main pieces of information that the data contain, but it is clear that by no means all the variance in the replies given by subjects has been accounted for.

II. METHOD

Readings were obtained from a wide variety of subjects totalling 182 in all. Of these, some were Church of England clergy, partly from Cambridge and partly from elsewhere, and some were ordinands from two theological colleges and one university college in Cambridge. Others were laymen, including both churchgoers¹ and non-churchgoers, of whom many were members of Cambridge University, but others came from different walks of life, mainly from a parish in north Kent. There was also a group of children from a Council School a short distance from Cambridge.

The material chosen for the experiment was prayers of the kind printed in books of prayers, as this not only seemed convenient to handle, but it was felt (and subsequently proved to be the case) that this kind of material would come within the experience of all the subjects, whether churchgoers or not.

There was some variation in the procedure adopted when giving the experiment, owing to variation in the circumstances in which subjects could be examined, but, in general, it was as follows and the variations from it do not appear to have affected the results:

Each subject was given the following four prayers printed on a sheet of paper:

- a. Help us, O Lord, to strive hard to control ourselves and to help our fellows that our lives may be useful in this world.
- b. Almighty and Everlasting God, we beseech thee that by the gracious gift of thy Spirit dwelling in our hearts, we may endeavour ourselves to follow after righteousness, and, loving all men, may ever seek the path of charitableness; for Jesus Christ's sake.
- c. Prevent us, O Lord, in all our doings with thy most gracious favour, and further us with thy continual help; that in all our works, begun, continued and ended in thee, we may glorify thy holy name; through Jesus Christ our Lord.
- d. Teach us, Almighty Father, to serve thee as thou deservest; to give and not to count the cost; to fight and not to heed the wounds; to toil and not to seek for rest; to labour and not to ask for any reward save that of knowing that we do thy will.

¹ Throughout this paper the term 'churchgoer' is intended to cover members of all denominations.

The subjects were then asked the following questions:

- In what order of preference do you place the prayers?
- What is the basis of your choice?
- Which (if any) of the prayers are familiar to you?
- To what extent are you a churchgoer? (State denomination.)
- Have you always belonged to the denomination of which you are now a member? If not, what was your former denomination?
- Have you had any experience of work with children or young persons?

The approximate age, and any other seemingly relevant details about the subjects were also recorded.

The last question about work with children was inserted as the result of a preliminary survey of the first forty or so results. Of these, several were from churchgoers who were schoolmasters or youth workers, and it soon became clear that their orders of choice showed considerable differences in some respects from those of other churchgoers. The data provided by the fifth question about change of denomination were not used, as the number who had changed from one denomination to another was very small.

In the case of the schoolchildren, the questions asked were somewhat simplified, as it was not thought at the time that they would be able to give reasons for their choices,¹ and some of the adult questions were irrelevant. They were asked, therefore, their order of preference, whether any of the prayers were familiar, whether they were in the habit of attending church, chapel or Sunday School, and whether their parents were in the habit of attending church or chapel. The data provided by this last question have not been used in the analysis of results, but it is perhaps interesting to note in passing that of the twenty-eight schoolchildren only four said they did not go to church, chapel or Sunday School, but fifteen said that their parents did not go to church or chapel.

Of the four prayers, **a** was composed for the experiment in as simple language as possible, care being taken not to use any words which might be considered archaic. **b** was also composed for the experiment and was intended to convey roughly the same meaning as **a**, but in traditional liturgical language, with archaic words purposely introduced. **c** is a very well-known prayer in the Church of England Prayer Book, with the last phrase of the original omitted. The contents of **a** and **b** were intended to be more or less similar to that of **c**. **d**, St Ignatius Loyola's prayer, was inserted as being a second traditional prayer of fairly similar content to **c**, but expressed in somewhat simpler and less 'liturgical' language—as being, in fact, a prayer which was traditional but at the same time comparatively free from archaisms.

As it turned out, this prayer material had several disadvantages, in particular the fact that the four prayers, besides differing from each other in the ways intended, also inevitably differed from each other in ways not intended, and that these latter differences, even if slight, were sometimes of the greatest importance and considerably affected the results of some of the subjects. In many ways it would have been better to have used portions of the Bible—the same passages in different translations—which would have enabled the subject-matter to be held constant while the language was varied.

The readings were all taken between the summers of 1938 and 1939.

¹ In a second experiment tried later which used the same technique, it was found that they were quite as capable of giving reasons as were adult subjects.

III. RESULTS

(1) *Preliminary description*

The subjects divided fairly clearly into three groups having differing degrees of church-going, thus:

- I. Clergy and ordinands.
- II. Lay churchgoers.
- III. Non-churchgoers.

They also divided fairly clearly into three age groups:

- A. Elder: 25 years and over.
- B. Younger: 18-24 years.
- C. Schoolchildren: 12-14 years.

As mentioned above, a preliminary inspection of the first results suggested that a third division should also be made, separating subjects who had had experience of social work with children, such as that involved in youth clubs, schools and Sunday Schools, from those who had not had such experience. The subjects were accordingly divided up into the classes listed in the first column of Table 1. In addition, there were two special groups listed at the foot of the table—Ordinands from College Z, and Quakers—which have been excluded from the general groups as their results showed certain special features and marked differences from those of other comparable subjects. They are dealt with separately later in the paper.

The divisions between some of these groups are, of course, arbitrary, and a certain number of borderline cases were a little difficult to place; but apart from these few cases, the divisions between the groups were fairly well marked, and in most cases it was quite clear to which group the subject belonged.

It will be noticed that no account is taken of the sex of the subjects. The clergy and ordinand classes obviously do not contain women, but there were a few women among the other classes. While further investigation with a larger sample might reveal some sex difference in the preferences, the women in the present sample are few in number and their preferences do not seem to differ noticeably from those of men. They have, therefore, been included in their appropriate classes without being distinguished from men.

As will be seen from the second column of Table 1 (see p. 69), the dividing up of the subjects in accordance with the three factors, 'Degree of Churchgoing', 'Age' and 'Work with Children', has reduced the size of some of the groups, especially those of the non-churchgoers, to one from which it is almost impossible to draw any valid conclusions. They are, however, included for the sake of completeness, and such inferences as can be drawn are discussed.

The middle columns of Table 1 show mean scores worked out from the placings. These are arrived at by allotting scores in accordance with Fisher & Yates's 'Scores for Ordinal Data' (2). Those for data in groups of four (as are those of the present experiment) are as follows:

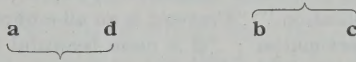
1st place	+1.03	3rd place	-0.30
2nd place	+0.30	4th place	-1.03

It has to be remembered that these mean scores really indicate the degree of unanimity amongst the members of a group rather than the intensity of the preferences of individual

subjects. It seems, however, not unreasonable to assume that there is a considerable correlation between unanimity within a group and strong preferences amongst the subjects which compose it.

The conversion of placings to these scores makes the assumption that the placings by a large number of subjects would follow a normal distribution. It does, however, give a convenient measure for comparison between groups and between prayers within any one group, and makes available a very much greater range of statistical methods for working out results. In treating the results of the present experiment χ^2 tests have been used whenever possible, but in some cases, where numbers have been insufficient for these to be used, other tests made possible by this method of scoring have been used instead.

The column next to the right-hand column of Table 1 gives the order of preference of each group. A bracket above the letters indicating the order of placing means that the prayers bracketed were placed equal. A bracket below the letters means that the difference in placing of the prayers at the extremities of the bracket is significant. Thus



means that prayer **a** was placed first, prayer **d** second, and prayers **b** and **c** equal last, and shows that the difference between the means of prayers **a** and **d** is significant (as, of course, is also the difference between the means of prayer **a** and prayers **b** and **c**), i.e. the difference between the means of these prayers is more than three times the standard error of the general mean of the group.

The last column of Table 1 shows the variance ratios worked out by the method of 'Analysis of variance', comparing for each group the variation between the mean scores of the different prayers with the variation between the individual subjects. Variance ratios which attain significance ($P < 0.05$) are printed in heavy type.

In Table 2 (p. 70) is shown a summary of the reasons given for choices. It will be seen that it is divided into four sections. The first column of the first section shows the total number of subjects in each group who gave 'Mode of expression' as a factor influencing their choice, irrespective of whether it was 'Beauty and dignity of expression' or 'Directness and simplicity of expression' which was concerned. These two factors are analysed in the two remaining columns of the first section.

The first column of the second section shows in a similar manner the number of those giving the subject-matter of the prayers as a factor influencing their choices, and the actual reasons given are analysed in the remaining columns of this section.

The third section contains columns for a number of miscellaneous reasons for choice.

It is important to note that several subjects gave more than one reason for choice, and that therefore the total number of reasons given cannot be expected to equal the number of subjects. It will be seen that among the lay churchgoers and non-churchgoers several subjects gave no reasons for choice. In all cases except one, this was because the circumstances in which the experiment was conducted made it impossible to ask for any reasons, and not because the subjects concerned were unable to state the basis of their choices. No attempt has been made to apply statistical methods to this table, as the pigeon-holing of the reasons is inevitably a somewhat subjective process for the experimenter, and for that reason liable to considerable error. The figures in the table

must therefore be taken with caution, and it should be realized that they can give only a rough indication of the factors governing choices.

To make clearer the type of reason indicated in each of the columns, examples may be given. It will be remembered that all are replies to the question, "What is the basis of your choice?"

'Beauty and dignity of expression': "The wording of **b** and **d** is particularly beautiful." "Beauty and rhythm of language." "Dignity of language." "Good-sounding English."

'Directness and simplicity of expression': "Concrete expression." "**a**, **d** chosen because of the clarity of expression and phraseology." "I prefer the simplest and most current language."

'Religious ideas': "Correct theological Christian thought." "**c** is theologically the most sound."

'Other ideas (including unspecified ideas)': "Not language so much as the thoughts behind it." "The moral and philosophical implications of the prayer **b**." "**a** gives chief emphasis on one's duty towards one's fellow-men."

'Practical value and "helpfulness"': "Classed in order of their meaning something practical, besides just being a series of good sounding phrases." "**c** is a source of strength, and an expression of self-devotion to Jesus Christ. It strikes a responsive chord within me." "I prefer **d** because it fits in with the difficulties of life."

'"Wideness" of subject-matter': "I like **c** because it is one of the few prayers which is so very secular, that is to say, wide in application." "Prayer **c** is an all-embracing prayer."

'Other affective aspects of subject-matter': "**d** is more beautiful than the others. The ideals mentioned appeal more." "**a** is concrete and fresh."

'Liking for some small point': "I liked the 'indwelling Spirit' idea in **b**."

'Dislike of some small point': "I don't like the word 'charitableness' in **b**." "The word 'prevent' in the third prayer is disconcerting." "I do not like the part in **d**, 'to fight and not to heed the wounds'."

'Familiarity liked': "**c** and **d** preferred to **b** probably because known." "**c** and **d** liked because of greater familiarity." "Choice on familiarity."

'Unfamiliarity liked': "**b** and **c** are too ordinary" (**b** and **c** were regarded as familiar, **d** was not). "**c** and **d** are familiar and I am rather tired of them."

'Associations': "**c** is a favourite prayer because it is the prayer of the guild to which I belong." **d** placed first "because I heard it often when I was comparatively young".

'Suitability for services': "Use in an ordinary mattins or evensong after the third collect." "**c** is unsatisfactory for children or an uneducated congregation."

(2) *The differences between groups having different degrees of churchgoing*

Table 3 (p. 71) is designed to show the differences between comparable groups with different degrees of churchgoing. The first of the two columns for each prayer contains the differences between the means of non-churchgoing and lay churchgoing groups, while the second contains the differences between the means of lay churchgoing and clergy-ordinand groups.

It can be seen at once that the largest and most consistent differences occur with prayers **a** and **c**. With the former there is in all cases except three, a fairly well-marked decrease in popularity with increase in churchgoing. Of the three differences between means which are exceptions to this general decrease, two are associated with very small groups, and the third is of negligible magnitude. For prayer **c** we may note, on the whole, a marked rise in popularity with increase in churchgoing. The three cases in which there is a fall are all associated with small and therefore unreliable groups.

Although there are fairly large differences in one or two cases with prayers **b** and **d**, the differences display little consistency and are associated with small groups. There would seem, however, to be a slight decrease in the popularity of prayer **b** as churchgoing increases, and a slight increase in the popularity of prayer **d** in the same direction.

A χ^2 test for prayer **a** was made of the results of the clergy and ordinands and the

lay churchgoers. For this purpose all the groups of § I of Table 1 were combined together, and all the groups of § II A and B of Table 1 were combined together. This was done in order to simplify the conditions of the test, and seemed a justifiable procedure for the following reason. As will be shown later, the differences between elder and younger groups (i.e. groups A and B) appear to be neither great nor consistent, and it therefore seems justifiable to combine them together. The combined groups of clergy and ordinands and of lay churchgoers thus produced are all of nearly the same size, and therefore the groups not working with children and the corresponding groups working with children (i.e. groups 1 and 2) have also been combined, as inaccuracies due to inequality in the sizes of these groups are likely to be negligible.

The placings for prayer **a** may therefore be summarized thus:

	1st place	2nd place	3rd place	4th place	Totals
Clergy and ordinands	4.3	3.4	12.8	35.5	56
Lay churchgoers	14	10	7.5	27.5	59
					115

For this table $\chi^2 = 8.73$, $P < 0.05$. The decimal points in the table are due to ties which are divided equally among the cells in which they fall, for instance a tie for third and fourth places is scored as 0.5 in each of the third and fourth cells.

The numbers of non-churchgoers are too small and their groups of too unequal size for a χ^2 test to be applied, but on a 't' test the differences between the means for elder non-churchgoers not working with children, and also for younger non-churchgoers not working with children, and those of their corresponding lay churchgoer groups are shown to be significant ($t = 4.33$, $P < 0.001$ and $t = 10.19$, $P \ll 0.001$ respectively).

Similar tests for prayer **c** also give significant results. The summary of placings is:

	1st place	2nd place	3rd place	4th place	Totals
Clergy and ordinands	22.8	22.9	8.3	2	56
Lay churchgoers	14.5	17	14	13.5	59
					115

For this table $\chi^2 = 12.59$, $P < 0.01$. As regards the differences between the means of non-churchgoing groups not working with children and the corresponding lay churchgoing groups, the results are for the elder groups $t = 3.26$, $P < 0.01$, and for the younger groups $t = 2.32$, $P < 0.05$.

The differences for prayers **b** and **d** are on the whole small and not significant.

In Table 4 (p. 71) an attempt is made to compare the types of reasons given for choices. In the first section of the table it will be noted that as churchgoing increases, the proportion of choices based upon the mode of expression rises as compared with the proportion based upon the subject-matter. The second section of the table indicates that there is a similar rise of stress on beauty and dignity of expression as compared with simplicity and directness. In the third section of the table what may be regarded as choices based on the more rational or intellectual features of the subject-matter are compared with those of a more affective kind. The first of each pair of figures for this section of the table is arrived at by adding together the second and third columns of the second section of Table 2. The second figure of each pair is obtained by adding together the fourth, fifth and sixth columns of the second section of Table 2. It will be noted that, on the whole, there is a rise in the stress on the more affective features of the subject-matter with increase in churchgoing.

As of the four prayers **a** is clearly the simplest and is the one which decreases in popularity with increase in churchgoing, and **c** is probably the best from a literary point of view—it is certainly better than **a** or **b**—and is the one which increases in popularity with increase in churchgoing, it can fairly be said that, taken as a whole, the evidence furnished by Table 3 tallies with that of Table 4 and suggests that as churchgoing increases, so the stress upon the beauty and dignity and the affective aspects generally of the prayers also increases. It may be noted that the generally high position and slight increase in popularity with churchgoing of prayer **d**, and the generally low position and slight decrease in popularity with churchgoing of prayer **b**, support the hypothesis that the increased stress on the affective elements is based upon a genuine appreciation of good literary form and not on any mere 'liturgical quality' of expression.

(3) *The differences between groups not working with children and the corresponding groups working with children*

In Table 5 (p. 71) are set out the differences between the means of groups not working with children and corresponding groups working with children. It will be noted that once again the main variation is in prayers **a** and **c**. Amongst churchgoers (i.e. groups I and II) prayer **a** shows a very marked rise in popularity amongst the groups working with children, and prayer **c** shows an almost equally marked fall in popularity. Prayer **b** shows a small decrease in popularity amongst lay churchgoers, and prayer **d** shows a small decrease in popularity amongst younger churchgoers.

With non-churchgoers, it is remarkable that all the tendencies amongst churchgoers appear to be reversed. It should, however, be remembered that the numbers of non-churchgoers working with children are very small, and that the means of these groups cannot be regarded as reliable. It is very unfortunate that at the time the readings were taken a greater effort was not made to obtain a larger number of non-churchgoing subjects, especially of those working with children.

χ^2 tests amongst churchgoers give significant results for prayers **a** and **c**. All the groups of churchgoers in §§ I and II A and B of Table 1 were combined together for the reasons stated in the discussion of Table 3. The placings for prayer **a** may be summarized as below:

	1st place	2nd place	3rd place	4th place	Totals
Not working with children	1	4	12	40	57
Working with children	17.3	9.4	8.3	23	58
					115

For this table $\chi^2 = 21.94$, $P < 0.001$. The placings for prayer **c** may be similarly summarized thus:

	1st place	2nd place	3rd place	4th place	Totals
Not working with children	27	17.5	9	3.5	57
Working with children	10.3	22.4	13.3	12	58
					115

For this table $\chi^2 = 13.24$, $P < 0.01$. The variations in the placing of prayers **b** and **d** are not significant.

In Table 6 (p. 72) an analysis is made of the reasons for choice given by the subjects in a manner similar to that of Table 4. From the first section of Table 6 we may notice that work with children appears to go with some increase in the number of choices based upon subject-matter as opposed to mode of expression. The second section of the table suggests

that work with children is accompanied by a considerable increase in the stress laid upon simplicity and directness of expression as opposed to beauty and dignity of expression. No figures can be given for non-churchgoers in column 2 of this section, as no non-churchgoers working with children gave 'mode of expression' as a reason for choice.

The third section of Table 6 compares the number of choices based upon the more affective elements of the subject-matter with those based upon the more intellectual. From the figures it would seem that there is little consistent variation between the two types of group in this respect.

The indications of Tables 5 and 6 together are that with work with children there goes, at any rate so far as churchgoers are concerned, a considerable increase in stress upon simplicity and directness. This is brought out by the rise in popularity among churchgoers of prayer **a**, and the fall in popularity of all the other prayers, especially **c**, and confirmed by the figures of the second section of Table 6, which show that many more subjects state 'simplicity and directness of expression' as a basis of choice amongst groups working with children than amongst the corresponding groups not doing so. For the non-churchgoing groups we may note that, while their results as shown by Table 5 are the reverse of those of churchgoers, their results in the first section of Table 6 are in line with those of churchgoers. As has been said before, it is almost impossible to make any reliable statement about these non-churchgoing groups as the numbers are too small.

(4) *The differences between groups of different ages*

In Table 7 (p. 72) the differences between the means of corresponding groups are set out in such a way as to make possible a comparison of different age groups. It should be noted that the two groups of schoolchildren—those attending Church or Sunday School and those attending neither—are, as it were, the forerunners respectively of the younger lay churchgoers and younger non-churchgoers, both those not working with children and also those working with children. The differences are therefore given between the means of the groups of schoolchildren and both classes of corresponding younger group. There are, of course, no schoolchildren who can properly be directly compared with ordinands.

Taking the table as a whole, it can be said that between the younger and elder groups (B and A) there are no large consistent differences, and in almost all cases the differences between individual pairs of groups are relatively small. There are, however, fairly well-marked differences between the churchgoing schoolchildren and the younger lay churchgoers not working with children. These differences consist of a rise in the popularity of prayers **b**, **c** and **d**, and a marked drop in the popularity of prayer **a**. On a 't' test the difference between the means for prayer **a** is significant: $t=4.88$, $P<0.001$. The choices of the younger lay churchgoers working with children appear to tally fairly closely with those of the schoolchildren themselves. We may note that in prayers **a**, **c** and **d** the advance in age from younger to elder is marked by differences which, although small, are in the same direction as those between schoolchildren and younger. In other words, while it may be said that churchgoers of about 18-24 years of age who are working with children display choices which correspond fairly closely with those of churchgoing schoolchildren, those who carry on this work beyond the age of 25 rather 'overdo' their choices.

With regard to the non-churchgoers, we may note the same sort of relationships between the various age groups, although the shifts between schoolchildren and their elders

are in directions different from those of the churchgoing groups. In spite of the small sizes of the groups the difference between the means for prayer **a** of non-churchgoing schoolchildren and younger non-churchgoers not working with children attains significance: $t=5.29$, $P<0.001$.

Although the differences between younger and elder are small and not consistent in the present sample, a closer study of the means set out in Table 1 suggests that with a larger sample it might be possible to see increase of age accompanied in some groups by a 'mellowing' of choices, and in others by a 'hardening'. An example of the former may be seen in the means for prayer **a** where the elder lay churchgoers not working with children give a very much smaller score than do their corresponding younger group. Evidence of 'hardening' may be seen in the case of prayer **c**, where in most cases the elder groups carry further the differences that we have seen going with different degrees of churchgoing and work with children. None of these differences are, however, significant, so that for the present they remain as the subjects of interesting speculation only.

It will be seen that on the whole there is some drop in the popularity of prayer **d** with increase in age. It seems possible that this may be due to prayer **d** being a somewhat more 'active' prayer than **b** or **c**, and thus having a greater appeal to youth. It is interesting to note that amongst lay churchgoers this fall is accompanied by a rise in the popularity of prayer **a**, which, while it also lays stress on 'service', has perhaps a less vigorous 'atmosphere' about it than prayer **d**.

In Table 8 (p. 73) the reasons for choices as between elder and younger groups are set out in a manner similar to that of Tables 4 and 6. It will be noted from the first section of the table that as between stress on 'mode of expression' and 'subject-matter' there is little consistent variation between the two age groups. In the second section it will be seen that as regards stress on 'beauty and dignity' as opposed to 'simplicity and directness', there is a close correspondence between the two age groups, except in the case of lay churchgoers not working with children. In the third section of the table we may note that there appears to be some rise in the stress upon the more affective features of the subject-matter in the case of the elder groups.

On the whole it seems clear that the assumption we made earlier, that there is little consistent difference between the younger and elder groups, is a justifiable one, and that any difference that comes about with age does so mainly during the middle teens. This would seem to be in line with the fact demonstrated by Starbuck(3) that the middle teens are the time of the most rapid religious development.

(5) *Special groups*

Ordinands from College 'Z'. The time has now come to consider the two special groups which have been left aside in the treatment of the general results. The twelve subjects from College 'Z', who we have previously noted all fall into the class 'Younger ordinands working with children', are remarkable not so much for the order of their choice as for the high degree of consistency within their group. All twelve put prayer **a** last, ten of the twelve put prayer **b** next to last, ten put prayer **c** first, and nine put prayer **d** second. This degree of uniformity is very much greater than that found amongst the other groups in general. By way of comparison, on the line in Table 1 below that recording the means of the subjects from College 'Z' are shown the means of the subjects from College 'Y' extracted from the group I B2 higher up the table. The differences

between the means for prayers **a** and **c** for these groups are significant. For prayer **a**, $t=2.6$, $P<0.02$ and for prayer **c**, $t=2.85$, $P<0.01$. Table 2 shows that amongst the subjects from College 'Z' there is, as compared with those from College 'Y', a somewhat greater stress upon subject-matter, upon beauty and dignity of expression, and upon the more affective aspects of the subject-matter. The indication would seem to be that some factor is operating in the case of College 'Z' which is absent in the case of College 'Y'. While the results do not indicate the nature of this factor, acquaintance with the colleges concerned suggests that it may be a 'strict atmosphere' and a considerable stress upon uniformity of doctrine and ecclesiastical method.

Quakers. We have noted previously that there seemed to be little difference between the choices given by members of different denominations, with the one exception of those given by Quakers. Of the five subjects in this group one was working with children, the rest not. The choices given by the group are remarkable for their order which is radically different from that of their nearest corresponding lay churchgoers' group, i.e. younger lay churchgoers not working with children (see Table 1). With the high placing of prayer **a** and the low placing of prayer **c**, they in a sense present an exaggerated form of the churchgoers working with children results. Extracting the one subject working with children (who produced the order **d b a c**), the differences between the means for prayers **a** and **c** of the remainder of the group and of the younger lay churchgoers not working with children, are significant. For prayer **a**, $t=11.16$, $P\ll 0.001$, and for prayer **c**, $t=3.44$, $P<0.01$. It will be noted from Table 2 that the stress seems to be upon simplicity and directness of expression and the more intellectual aspects of the subject-matter.

(6) *The effect of familiarity*

Table 2 shows that several of the subjects were influenced in their choice by a liking for the familiarity of one or more of the prayers or by the associations they aroused. A few subjects expressed a preference for novelty, but it will be seen that these were many less than those who expressed a liking for familiarity. It may be noted that the traditional prayers **c** and **d** were not always regarded as familiar and, at the same time, the prayers **a** and **b** were in some cases regarded as familiar, although, as they were composed for the experiment, they could not have been so. A table of the mean scores for the four prayers to show the differences going with familiarity is given below. It will be seen that in all cases the mean score when a prayer is regarded as familiar is higher than the mean score when it is not so regarded.

	Prayer a	Prayer b	Prayer c	Prayer d
When not regarded as familiar	-0.277	-0.178	-0.360	+0.160
When regarded as familiar	+0.203	-0.035	+0.350	+0.342
Differences	+0.480	+0.143	+0.710	+0.182

χ^2 tests of the association between placing and familiarity give significant results for prayers **c** and **d**, and results of borderline significance for prayer **a**. For these tests all groups were combined and the cells were further combined to make fourfold tables. The relevant figures are:

		Prayer a		Prayer b		Prayer c		Prayer d	
Places	...	1, 2, 3	4	1, 2	3, 4	1, 2	3, 4	1, 2	3, 4
Familiar		9	2	13.8	18.2	89.2	31.8	94	31
Unfamiliar		86	85	46	104	17.6	43.4	32.6	24.4
		$\chi^2=4.12$		1.86		33.67		6.00	
		$P<0.05$		0.2		0.001		0.02	

It can therefore be said with some confidence that the prayers are placed higher when they are regarded as familiar. It is important to remember, however, that this does not mean that we can say that familiarity is the cause of this higher placing, for it is clear that familiarity is correlated with other factors. For instance, in the case of the traditional prayers **c** and **d**, familiarity is obviously correlated with degree of churchgoing. Even in the case of prayers **a** and **b**, the greater knowledge possessed by churchgoers of the contents of prayer books and of the prayers normally used in church would give them a greater chance of recognizing their novelty and therefore of not erroneously regarding them as familiar.

It is possible, but the data do not seem to be sufficient to give a clear indication, that the changes associated with these other factors, for instance the increase of popularity of prayer **c** with increase of churchgoing, are largely due to familiarity. While this is probably the case to some extent, it must also be remembered that prayers **c** and **d** are undoubtedly better English and probably better theology than prayers **a** and **b**.

It seems, therefore, that the association with familiarity indicated by the differences in the means shown above is of relatively little interest, and that some method of treating the results must be adopted which gets rid of the effect of the other factors being studied, especially 'degree of churchgoing'. Figures were accordingly worked out to make a fourfold contingency table for each of the prayers. The first cell of each table contained the number of cases in which the prayer concerned was regarded as familiar and was placed higher than the mean of its group, each of the groups listed in Table 1 (except ordinands from College 'Y') being treated as a separate unit. The second cell contained the number of times the prayer was regarded as familiar and placed below the mean of its group. The third cell contained the number of times the prayer was regarded as unfamiliar and placed above the mean of its group, and the fourth cell contained the number of times the prayer was regarded as unfamiliar and placed below the mean of the group. For each prayer the figures in the first and fourth cells were higher than expectation, and those in the second and third lower, indicating a positive association between familiarity and placing higher than the mean of the group. This association for prayers **a**, **b** and **d** was very small, but for prayer **c** it was large and highly significant. The table for prayer **c** is given below:

	Placed above mean of group	Placed below mean of group	Totals
Regarded as familiar	79	42	121
Not regarded as familiar	17	44	61
			182

For this table $\chi^2 = 22.78$, $P \leq 0.001$.

From the evidence it seems clear that familiarity is, on the whole, a factor making for popularity, that in one case it is an important one, and that its influence outweighs any preference for novelty when readings are taken over a number of subjects. It would seem perhaps justifiable to generalize by saying that the evidence suggests that some prayers derive a great part of their popularity from their familiarity and, while other prayers are preferred mainly for other reasons, when they are familiar the preference for them is increased.

(7) *The effect of archaic language*

It is remarkable that only a very few of the subjects stated a dislike of archaic language, and that none stated a positive liking for it. It may be that an unexpressed

preference for traditional liturgical language is a cause of part of the preferences shown by churchgoers. In support of this view may be quoted the fact that the great majority of clergy, ordinands and lay churchgoers prefer prayer **c** to prayer **d**, and prayer **b** to prayer **a**; i.e. of both the pair of familiar prayers and the pair of unfamiliar ones, the prayer expressed in the more 'liturgical' language is preferred. It seems likely, however, that in the determination of choices the archaism or modernity of the language employed plays a relatively small part compared with other factors such as real literary quality, simplicity and directness, and the various aspects of the subject-matter. It may be noted in evidence for this view that prayer **b** is generally placed low, and prayer **d** is generally placed high, although prayer **b** is written in a very much more 'liturgical' language than prayer **d**.

IV. SUMMARY OF CONCLUSIONS

We may sum up by saying that in this experiment 182 subjects were given four prayers of differing language and somewhat differing subject-matter, and asked to place them in order of preference.

Statistical analysis of the orders of preference given indicated that most of the groups of subjects displayed significant group preferences which differed widely between one group and another. These orders of preference and the reasons for them given by the subjects suggest the following conclusions:

(1) With increase in churchgoing there goes increased stress upon the mode of expression of the prayers as opposed to the ideas contained in them (subject-matter), upon beauty and dignity of expression as opposed to simplicity and directness, and upon the more affective aspects of the subject-matter as opposed to the more intellectual ones. Most non-churchgoers showed a preference for the most simple and direct of the prayers. Clergy and ordinands showed strong preferences for the traditional liturgical prayers, and strong dislike of the prayer preferred by the non-churchgoers. The preferences of lay churchgoers were intermediate. There were significant differences of choice, not only between non-churchgoers and lay churchgoers, but also between lay churchgoers and clergy and ordinands.

(2) Amongst churchgoers who were doing social work with children, there was a considerably increased stress laid upon simplicity and directness of expression, and a strong liking for the more simple and direct prayers. On the whole, the preferences displayed by subjects working with children were found to tally with those of school-children.

(3) There was little consistent difference of choice between the age group 18-24 years and those over 25, but there were considerable differences between schoolchildren (12-14 years) and those of the 18-24 years age group not engaged in work with children.

(4) It appeared that the 'atmosphere' of their college exerted a considerable effect upon the choices of one group of ordinands.

(5) A much greater preference for simple prayers was shown by Quakers than by churchgoers of other denominations.

(6) The preference for a prayer seemed to be increased when it was regarded as familiar or aroused associations. In the case of one of the prayers, the effect of familiarity appeared to be very marked. In that of the others the effect was small, but consistently positive.

(7) The question of whether a prayer was expressed in old-fashioned or in modern language appeared to be unimportant compared with other factors.

I am indebted to Mr J. Wishart of the School of Agriculture, Cambridge, for assistance with the method of 'Analysis of Variance'. My thanks are also due to the authorities of the Psychological Laboratory and two Theological Colleges in Cambridge who granted facilities for the obtaining of results from groups of students, and to the Headmaster of a Cambridgeshire school who gave permission for results to be obtained from one of his classes. Finally, I would record my gratitude to the individual subjects for their painstaking co-operation.

Table 1. *General results: orders of placing*

Group	No. of subjects in group	Mean scores			Order of placing	Variance ratios
		Prayer a	Prayer b	Prayer c	Prayer d	
I. Clergy and ordinands:						
A. Clergy and elder ordinands						
1. Not working with children	12	-0.828	-0.206	+0.707	+0.327	12.86
2. Working with children	10	-0.412	-0.133	+0.133	+0.412	1.75
B. Younger ordinands						
1. Not working with children	15	-0.787	-0.237	+0.561	+0.463	15.40
2. From Colleges 'X' and 'Y', working with children	19	-0.409	-0.231	+0.330	+0.310	4.18
II. Lay churchgoers:						
A. Elder						
1. Not working with children	13	-0.471	-0.028	+0.373	+0.126	2.03
2. Working with children	15	+0.472	-0.317	-0.352	+0.197	4.06
B. Younger						
1. Not working with children	17	-0.866	+0.068	+0.295	+0.503	14.51
2. Working with children	14	+0.043	-0.181	-0.190	+0.328	1.09
C. Schoolchildren attending Church or Sunday School	24	+0.039	-0.322	-0.080	+0.363	2.72
III. Non-churchgoers:						
A. Elder						
1. Not working with children	10	+0.751	-0.107	-0.609	-0.035	6.84
2. Working with children	3	-0.143	+0.143	-0.343	+0.343	0.27
B. Younger						
1. Not working with children	6	+0.908	-0.106	-0.465	-0.343	5.15
2. Working with children	3	+0.342	-0.114	-0.114	-0.114	0.17
C. Schoolchildren not attending Church or Sunday School	4	-0.698	+0.333	+0.1825	+0.1825	1.22
Special groups:						
Ordinands (all younger working with children) from College 'Z', Similar ordinands from College 'Y' (taken from I B2 above)	12	-1.030	-0.139	+0.908	+0.261	65.97
Quakers	16	-0.550	-0.126	+0.410	+0.266	4.56
	5	+0.472	+0.014	-0.958	+0.472	6.16

Table 2. *General results: reasons given for choices*

Group	Mode of expression					Subject-matter						Other reasons				No reason given
	Mode of expression given as a reason for choice	Beauty and dignity	Directness and simplicity	Subject-matter given as a reason for choice	Religious ideas	Other ideas	Practical value and "helpfulness"	"Wideness"	Other affective aspects	Liking for some small point	Dislike of some small point	Familiarity liked	Unfamiliarity liked	Associations	Suitability for services	
I. Clergy and ordinands:																
A. Clergy and elder ordinands																
1. Not working with children	7	7	•	5	2	1	1	3	•	•	1	4	•	3	1	•
2. Working with children	7	4	4	9	2	3	3	1	1	•	2	1	•	•	•	•
B. Younger ordinands																
1. Not working with children	9	9	2	11	5	2	2	•	4	2	•	3	•	1	1	•
2. Working with children, from Colleges 'X' and 'Y'	14	7	8	15	7	3	1	1	4	2	1	2	•	1	3	•
II. Lay churchgoers:																
A. Elder																
1. Not working with children	3	3	3	6	1	3	3	•	•	•	•	•	2	1	•	3
2. Working with children	2	1	2	7	1	1	4	•	2	•	•	•	•	•	•	7
B. Younger																
1. Not working with children	12	12	3	10	2	5	•	1	2	•	2	3	•	•	2	2
2. Working with children	3	1	2	5	2	2	2	•	1	•	1	1	1	1	•	8
C. Schoolchildren attending Church or Sunday School	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	24
III. Non-churchgoers:																
A. Elder																
1. Not working with children	4	1	3	7	1	5	2	1	•	•	•	•	•	•	•	1
2. Working with children	•	•	•	2	1	1	•	•	•	•	•	•	•	1	•	•
B. Younger																
1. Not working with children	2	•	2	4	2	2	1	•	•	1	•	•	•	•	•	1
2. Working with children	•	•	•	3	1	2	•	•	•	•	•	•	•	•	•	•
C. Schoolchildren not attending Church or Sunday School	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	4
Special groups:																
Ordinands from College 'Z'	6	6	2	12	8	•	4	1	5	•	•	1	•	1	•	•
Similar ordinands from College 'Y'	11	5	6	14	6	3	1	1	4	2	1	2	1	1	3	•
(taken from I B2 above)																
Quakers	2	•	2	4	1	2	1	•	•	1	1	•	•	1	•	•

Table 3. *Table of differences between mean scores to show the differences between groups having different degrees of churchgoing*

III = non-churchgoers. II = lay churchgoers. I = clergy and ordinands.

	Prayer a		Prayer b		Prayer c		Prayer d	
	III~II	II~I	III~II	II~I	III~II	II~I	III~II	II~I
A1. Elder, not working with children	-1.222	-0.357	+0.079	-0.178	+0.982	+0.334	+0.161	+0.201
A2. Elder, working with children	+0.615	-0.884	-0.460	+0.184	-0.009	+0.485	-0.146	+0.215
B1. Younger, not working with children	-1.774	+0.079	+0.168	-0.305	+0.760	+0.266	+0.846	-0.040
B2. Younger, working with children	-0.299	-0.452	-0.067	-0.050	-0.076	+0.520	+0.442	-0.018
C. Schoolchildren	+0.737	—	-0.655	—	-0.2625	—	+0.1805	—

Table 4. *Reasons for choices; table to show the differences between groups having different degrees of churchgoing*

III = non-churchgoers. II = lay churchgoers. I = clergy and ordinands.

The ratios of numbers giving mode of expression (α), and numbers giving subject-matter (β) as a reason for choice.

	III	II	I
	$\alpha : \beta$	$\alpha : \beta$	$\alpha : \beta$
A1. Elder, not working with children	4 : 7	3 : 6	7 : 5
A2. Elder, working with children	0 : 2	2 : 7	7 : 9
B1. Younger, not working with children	2 : 4	12 : 10	9 : 11
B2. Younger, working with children	0 : 3	3 : 5	14 : 15

The ratios of numbers giving beauty or dignity of expression (γ), and numbers giving simplicity or directness of expression (δ) as a reason for choice.

	III	II	I
	$\gamma : \delta$	$\gamma : \delta$	$\gamma : \delta$
A1. Elder, not working with children	1 : 3	3 : 3	7 : 0
A2. Elder, working with children	—	1 : 2	4 : 4
B1. Younger, not working with children	0 : 2	12 : 3	9 : 2
B2. Younger, working with children	—	1 : 2	7 : 8

The ratios of the numbers of times more affective features of the subject-matter (helpfulness, wideness or other affective aspects) are given as reasons for choice (ϵ), and the numbers of times religious or other ideas are given as reasons for choice (ζ).

	III	II	I
	$\epsilon : \zeta$	$\epsilon : \zeta$	$\epsilon : \zeta$
A1. Elder, not working with children	3 : 6	3 : 4	4 : 3
A2. Elder, working with children	1 : 1	6 : 2	5 : 5
B1. Younger, not working with children	1 : 4	3 : 7	6 : 7
B2. Younger, working with children	0 : 3	3 : 4	6 : 10

Table 5. *Table of differences between mean scores to show the differences between groups not working with children and the corresponding groups working with children*

1 = not working with children. 2 = working with children.

	Prayer a	Prayer b	Prayer c	Prayer d
	1~2	1~2	1~2	1~2
IA. Clergy and elder ordinands	+0.416	+0.073	-0.574	+0.085
IB. Younger ordinands	+0.378	+0.006	-0.231	-0.153
IIA. Elder lay churchgoers	+0.943	-0.289	-0.725	+0.071
IIB. Younger lay churchgoers	+0.909	-0.249	-0.485	-0.175
IIIA. Elder non-churchgoers	-0.894	+0.250	+0.266	+0.378
IIIB. Younger non-churchgoers	-0.566	-0.014	+0.351	+0.229

Table 6. *Reasons for choices: table to show the differences between groups not working with children and the corresponding groups working with children*

1=not working with children. 2=working with children.

The ratios of numbers giving mode of expression (α), and numbers giving subject-matter (β) as a reason for choice.

	1	2
	$\alpha : \beta$	$\alpha : \beta$
IA. Clergy and elder ordinands	7: 5	7: 9
IB. Younger ordinands	9: 11	14: 15
IIA. Elder lay churchgoers	3: 6	2: 7
IIB. Younger lay churchgoers	12: 10	3: 5
IIIA. Elder non-churchgoers	4: 7	0: 2
IIIB. Younger non-churchgoers	2: 4	0: 3

The ratios of numbers giving beauty or dignity of expression (γ), and numbers giving simplicity or directness of expression (δ) as a reason for choice.

	1	2
	$\gamma : \delta$	$\gamma : \delta$
IA. Clergy and elder ordinands	7: 0	4: 4
IB. Younger ordinands	9: 2	7: 8
IIA. Elder lay churchgoers	3: 3	1: 2
IIB. Younger lay churchgoers	12: 3	1: 2
IIIA. Elder non-churchgoers	1: 3	—
IIIB. Younger non-churchgoers	0: 2	—

The ratios of the numbers of times more affective features of the subject-matter (helpfulness, wideness or other affective aspects) are given as reasons for choice (ϵ), and the numbers of times religious or other ideas are given as reasons for choice (ζ).

	1	2
	$\epsilon : \zeta$	$\epsilon : \zeta$
IA. Clergy and elder ordinands	4: 3	5: 5
IB. Younger ordinands	6: 7	6: 10
IIA. Elder lay churchgoers	3: 4	6: 2
IIB. Younger lay churchgoers	3: 7	3: 4
IIIA. Elder non-churchgoers	3: 6	1: 1
IIIB. Younger non-churchgoers	1: 4	0: 3

Table 7. *Table of differences between mean scores to show the differences between groups of different ages*

C=schoolchildren (12-14). B=younger (18-24). A=elder (25 and over).

	Prayer a		Prayer b		Prayer c		Prayer d	
	C~B	B~A	C~B	B~A	C~B	B~A	C~B	B~A
II. Clergy and ordinands, not working with children	—	-0.041	—	+0.031	—	+0.146	—	-0.136
I2. Clergy and ordinands, working with children	—	-0.003	—	+0.098	—	-0.197	—	+0.102
III1. Lay churchgoers, not working with children	-0.905	+0.395	+0.390	-0.096	+0.375	+0.078	+0.140	-0.377
II2. Lay churchgoers, working with children	+0.004	+0.429	+0.141	-0.136	-0.110	-0.162	-0.035	-0.131
III1. Non-churchgoers, not working with children	+1.606	-0.157	-0.433	-0.007	-0.6475	-0.144	-0.5255	+0.308
III2. Non-churchgoers, working with children	+1.040	-0.485	-0.447	+0.257	-0.2965	-0.229	-0.2965	+0.457

Table 8. *Reasons for choices: table to show the differences between groups of different ages*

B=younger (18-24). A=elder (25 and over).

The ratios of numbers giving mode of expression (α), and numbers giving subject-matter (β) as a reason for choice.

		B	A
		$\alpha : \beta$	$\alpha : \beta$
I. Clergy and ordinands:	1. Not working with children	9 : 11	7 : 5
	2. Working with children	14 : 15	7 : 9
II. Lay churchgoers:	1. Not working with children	12 : 10	3 : 6
	2. Working with children	3 : 5	2 : 7
III. Non-churchgoers:	1. Not working with children	2 : 4	4 : 7
	2. Working with children	0 : 3	0 : 2

The ratios of numbers giving beauty or dignity of expression (γ), and numbers giving simplicity or directness of expression (δ) as a reason for choice.

		B	A
		$\gamma : \delta$	$\gamma : \delta$
I. Clergy and ordinands:	1. Not working with children	9 : 2	7 : 0
	2. Working with children	7 : 8	4 : 4
II. Lay churchgoers:	1. Not working with children	12 : 3	3 : 3
	2. Working with children	1 : 2	1 : 2
III. Non-churchgoers:	1. Not working with children	0 : 2	1 : 3
	2. Working with children	—	—

The ratios of the numbers of times more affective features of the subject-matter (helpfulness, wideness or other affective aspects) are given as reasons for choice (ϵ), and the numbers of times religious or other ideas are given as reasons for choice (ζ).

		B	A
		$\epsilon : \zeta$	$\epsilon : \zeta$
I. Clergy and ordinands:	1. Not working with children	6 : 7	4 : 3
	2. Working with children	6 : 10	5 : 5
II. Lay churchgoers:	1. Not working with children	3 : 7	3 : 4
	2. Working with children	3 : 4	6 : 2
III. Non-churchgoers:	1. Not working with children	1 : 4	3 : 6
	2. Working with children	0 : 3	1 : 1

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A COMPARATIVE STUDY OF ANNOYANCES

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I. INTRODUCTION

So many unknown conditions enter into the production of any psychological test score that it often seems impossible to determine what, in fact, the test is measuring. Factor analysis may do much to establish the range of some factors, but the psychological interpretation of these statistical solutions is often very difficult and arbitrary.

For this reason, many psychologists are led to an empirical extreme, whereby the only permissible statement of the function of a test is in terms of its predictive value. We can say how likely a man with a given set of test scores is to be successful in various occupations, but any psychological interpretation of these empirical findings must be rigidly eschewed as merely speculative. The terms of reference of psychology are thus reduced to two kinds: behaviour in life situations and behaviour in test situations. And the function of psychology is limited to developing statistical techniques whereby the one can be predicted from the other.

In contradistinction to this view, workers along different lines consider that the proper concern of psychology is with the way in which individuals think, feel and strive and that if the statistical approach is incapable of arriving at valid conclusions in these terms, then it is fundamentally inadequate.

A cleavage of opinion seems therefore to have developed between those who regard psychological arguments as the congenial but unscientific pastime of armchair mystics and those who consider the statistical method an elegant but ultimately irrelevant form of intellectual gymnastics.

It seems, however, that if the psychology of temperament is to develop as a science, the two approaches must converge. We must admit the right of psychology to argue in its own terms of reference, while deriving what benefit it can from the more rigorous inductive methods.

The present investigation is a very limited attempt to derive psychologically meaningful results from experimental data by a combination of arguments of both kinds.

II. EXPERIMENT

The material consisted of five paper and pencil tests of identical form, the various items of which were presented together in a random order. The tests are composed each of twelve descriptions of possibly annoying situations of a defined type. The subject is, however, unaware of the classification and is merely asked to record which of the sixty

situations annoy him. He is told that he may record as many or as few items as he pleases and that the purpose of the enquiry is to find out which are the most common everyday irritations. It is hoped that the purpose of the tests is thus sufficiently obscured from the subjects and that relatively guileless responses may therefore be expected. No time limit is imposed.

In considering results no attempt will be made to interpret scores on any single test, since its psychological significance cannot be assessed with any confidence owing to the multitude of factors which may be involved. The separate tests are, however, designed as controls upon one another and it is thought that comparisons between them should afford a valid estimate of the relative affective value of various kinds of situations to various kinds of subjects. It is hoped that relatively vulnerable or sensitive points may thus be detected in the psychological make-up of the individuals tested and that these may help towards explaining why individuals of some kinds break down under stresses which are normally tolerable.

The subjects used were 250 service hospital patients, half of whom were diagnosed as psychoneurotics and half of whom had no record of neurotic disorder. Anxiety was by far the most frequent diagnosis among the neurotics. A survey of the general population from which the neurotic sample was drawn is given by Slater⁽¹⁾. Few of this group had been subjected to battle stress and the great majority was regarded by the psychiatrists as suffering from chronic personality defects. It is hoped that a comparison of scores of these two groups may indicate some of the ways in which these defects are made manifest.

III. TESTS

(a) *Test items*

The selection of items was based on previous trials in which provisional tests were administered individually to smaller groups of neurotic and normal soldiers, who were asked for introspections, comments and additions. In the preliminary sessions subjects were asked to discuss the situations described freely as it was thought possible in this way to eliminate such items as were of no interest to the soldiers or of which the significance was ambiguous, so arriving at tests which had the necessary unequivocal appeal to the groups under investigation. Since there is no reason to suppose that the groups being compared differ in social background or in objective experience of these situations, comparisons are considered to be valid within the limits of the case material. Since the selection of test items is still to some extent subjective, it is thought best to show the tests in full, so that any other possible principles of classification may be open to inspection. The numbers before items indicate the random order in which they were presented.

Test 1

2. To be kept waiting for an appointment.
4. A rude remark about a member of your family.
11. Someone who tries to swing work on you.
13. Someone who pushes in front of you in a queue.
17. You have to wait a long time to use a public telephone.
27. You are overcharged for something in a shop.
31. Someone who borrows one of your possessions without asking permission.
33. Someone who interferes with you while you are working.

- 35. A just complaint is ignored.
- 37. Someone who wastes your time.
- 48. You are told a stupid lie.
- 52. Important family news is kept from you.

Test 2

- 6. A stupid joke is made at your expense.
- 7. Unnecessary orders.
- 12. Your word is doubted.
- 20. A rude remark about your personal appearance.
- 29. Somebody who does not listen to what you are saying.
- 39. Someone who keeps picking holes in your work.
- 43. Someone who tells you how to do your job.
- 46. Unpleasant gossip about your personal affairs.
- 51. Someone who keeps forgetting your name.
- 54. Being watched.
- 57. You are accused of something you haven't done.
- 59. Having your leg pulled.

Test 3

- 3. You find you have made a silly mistake.
- 8. You forget what you were just going to say.
- 9. Losing your way.
- 16. You have too many things to do.
- 19. You are unable to sleep.
- 22. You mislay one of your possessions.
- 25. Travelling in a crowded railway carriage.
- 34. You can't remember a name which is on the tip of your tongue.
- 42. You find you are late for an appointment.
- 45. You forget what you are looking for.
- 47. Oversleeping.
- 50. You fail at something which you usually do well.

Test 4

- 1. A man who sucks his teeth.
- 10. Someone with very bad teeth.
- 15. A torn newspaper.
- 18. Spitting in the street.
- 23. Badly cooked food.
- 28. A dirty room.
- 36. Greasy knives and forks.
- 40. An unmade bed.
- 44. Someone picking his nose.
- 49. Someone who bites his nails.
- 56. A child with a dirty face.
- 60. You are given food on a dirty plate

Test 5

- 5. Someone switches on the wireless when you want to be quiet.
- 14. A pneumatic drill.
- 21. Rattling windows.
- 24. A dog barks continuously.
- 26. A creaking bed.
- 30. The sound of a knife grating on a plate.
- 32. A banging door.
- 38. Trampling overhead.
- 41. A noisy argument which you can't avoid.
- 53. The sound of hammering.
- 55. A jazz band.
- 58. Someone tapping on the table

(b) *Definitions*

The definitions to which the items are intended to conform are as follows:

Test 1. Situations involving threats to objective interests and possibly, but not necessarily, to self-esteem. The annoyance is directed against others.

Test 2. Situations involving threats to self-esteem, but not necessarily to objective interest. The annoyance is directed against others.

Test 3. Situations involving either self-esteem or objective interest. The annoyance is directed against the self.

Test 4. Impersonal annoyance due to offensive sights.

Test 5. Impersonal annoyance due to offensive sounds.

IV. RESULTS

Table 1 shows and compares average scores for neurotic and normal subjects on each test. It can be seen that neurotics score more on tests 2, 3 and 5 and that differences on tests 1 and 4 are insignificant.

Table 1. *Comparison of means*

Tests	Normal		Neurotic		Diff.	<i>t</i>	<i>p</i>
	Mean	<i>s</i>	Mean	<i>s</i>			
1	8.072	2.976	8.264	2.871	0.192	0.52	0.6
2	6.712	2.850	7.760	2.367	1.048	3.16	0.01
3	5.216	3.257	6.992	2.816	1.776	4.61	0.001
4	7.408	2.518	6.952	2.831	-0.456	1.35	0.2
5	4.048	3.265	6.728	3.606	2.680	6.16	0.001

Table 2 shows the with-in group product-moment correlations between the tests. Since all values for *r* over 0.25 are highly significant ($P=0.01$) it can be seen that none of these values is attributable to chance. It is therefore shown that there is a marked tendency for the subject who scores highly on one test to score highly on any other.

Table 2. *Correlations between tests*

Tests	1	2	3	4	5
1	—	0.6862	0.6099	0.4560	0.3802
2	—	—	0.5919	0.3613	0.4930
3	—	—	—	0.4341	0.5092
4	—	—	—	—	0.4892

This accords with the finding of Conrad, Jones & Carter(2) with a similar battery of a 'general factor of annoyability' within the limits of the material. No conclusion is drawn from this here, however, since a great many indefinable influences may be involved in the general set towards the tests and no attempt has been made to validate them as tests of annoyability as such. It might be true, for example, that the neurotic is actually more irritable to unpleasant stimuli in general or that he is more ready to express his irritation in this way than is the normal man. If this were so, the general tendency to score highly could be interpreted as abnormal and the neurotics might be shown to be specifically insensitive to the control sets of situations. On the other hand, if the general factor were assumed to operate on the same level in the two groups, then the results shown in Table 1 could be taken at their face value. This ambiguity does not affect the argument however, if conclusions are based on comparisons between tests, since only relative values are then taken into account.

V. DISCUSSION OF 'PERSONAL ANNOYANCES'

(a) Comparison between tests 1 and 2

These tests clearly have much in common psychologically in addition to the general tendency referred to above, since both involve aspects of self-assertion; and this is confirmed statistically by the coefficient of correlation between them, which is the highest in the table. The second, however, discriminates, whereas the first does not. It, therefore seems reasonable to attribute the discriminating power of the second to those qualities in which it differs from the first; these relations can be represented statistically by the following analysis given in Table 3. For statistical methods used in this paper see reference (3).

The analysis shows that when due allowance is made for scores on the first test, the probability of the observed differences on the second occurring by chance becomes extremely remote. It can therefore be stated, subject to the correctness of the definitions, that this comparison shows the neurotic's relatively greater sensitivity to assaults upon his self-esteem rather than to those upon his objective interests. The problem of interpreting the observed difference will therefore be attempted on this basis.

Table 3. *Analysis of variance of scores on test 2 corrected for test 1*

	Sum squares	D.F.	Mean square	<i>t</i>	<i>p</i>
Between groups	53.984	1	53.984	3.84	0.001
Within groups	901.964	247	3.652		
Total	955.948	248			

The most obvious explanation seems to be that an abnormal score indicates the individual whose interest and attention are customarily absorbed to a relatively greater extent in maintaining his idea of himself than in adapting to objective situations. It is thought that this reaction may best be understood as an anxiously defensive one based on an underlying awareness of inadequacy. An alternative interpretation is that the neurotic is a narcissist, so absorbed in loving admiration of himself that any stimulus tending to interfere with this enjoyable pursuit is perceived more readily and reacted to more emotionally than by the normal man. The former interpretation seems psychologically more probable and in keeping with psychiatric opinion generally. Both are, however, consistent with the general view that the neurotic's interest tends abnormally in the direction of egocentricity.

(b) Comparison between tests 1 and 3

A similar argument to the above may be pursued in comparing these two tests. The high value of the correlation coefficient confirms the supposition that they have much in common psychologically; the points of difference between them can be shown to account adequately for the discriminating power of the third. In this case the analysis is as shown in Table 4.

Table 4. *Analysis of variance of scores on test 3 corrected for test 1*

	Sum squares	D.F.	Mean square	<i>t</i>	<i>p</i>
Between groups	170.818	1	170.818	5.407	0.001
Within groups	1443.429	247	5.844		
Total	1614.247	248			

The analysis shows that, when due allowance is made for scores on the first test, the significance of the difference between groups on the third is beyond reasonable doubt. An attempt may therefore be made to interpret this finding in the light of the hypothetical content of the tests. If the definitions be accepted, the essential difference between the first and the third test is in the direction of the annoyance whether this be against others or against the self. On this basis the neurotics could be said to show a relatively greater tendency to the subjective rather than to the objective reaction as compared with the normal man.

(c) *Comparison of test 3 with tests 1 and 2*

If the above distinction is valid, however, the third test should also discriminate between the two groups independently of those qualities which it shares with the second, since the latter consists also of situations in which the annoyance is directed against others. That this should be the case is by no means obvious, since both tests differentiate independently and may be doing so for the same reason. Moreover, an analysis of the variance of the third test indicates that it has some qualities in common with the second which it has not in common with the first. The statistical argument in this case is as shown in Table 5.

Table 5. *Analysis of variance of uncorrected scores on test 3*

	Sum squares	D.F.	Mean square	<i>t</i>	<i>p</i>
Due to regression on tests 1 and 2	985.444	2	492.722	—	—
Due to regression on test 1 alone	854.731	1	854.731	—	—
Additional effect due to test 2	130.713	1	130.713	4.95	0.001
Due to differences between groups	197.136	1	197.136	—	—
Error	1312.716	246	5.336	—	—
Total	2495.296	249			

The comparison made on the third line shows that a significant reduction in the error variance of test 3 is effected when test 2 is controlled as well as test 1 and this suggests a psychological overlap between tests 2 and 3. If this argument be accepted, the test material can be examined for qualities which are shared by the second and third tests, but not by the first. The interpretation offered on this basis is that both tests 2 and 3 measure the subject's sensitivity to stimuli tending to remind him of his own inadequacies. That such an interpretation could account, at least partially, for the discriminating power of both tests is considered psychologically feasible, since the neurotics might be expected to have a heightened awareness in this direction. The hypothesis cannot therefore be ruled out, that the discriminating power of the third test is partly due to those of its qualities in which it resembles the second. That is, that it discriminates in so far as it affords a measure of the subject's relatively greater interest in maintaining his idea of himself than in adapting to objective situations.

The sufficiency of this hypothesis can, however, be submitted to a statistical test as shown in Table 6.

Table 6. *Analysis of variance of scores on test 3, corrected for tests 1 and 2 together*

	Sum squares	D.F.	Mean square	<i>t</i>	<i>p</i>
Within group	1312.716	246	5.3361	—	—
Between group	99.577	1	99.577	4.32	0.001
Total	1412.293	247			

The analysis shows the hypothesis to be insufficient since, when due allowance is made for scores on the first two tests, the difference between groups on the third is still maintained although the slightly diminished value of *t* suggests that some loss in discriminating power may have been entailed by the procedure.

The original hypothesis implied in the definitions is therefore confirmed and it can be stated that, within the limits of the material, the neurotics show a relatively greater tendency to be annoyed with themselves than with others, as compared with the normal men.

Several interpretations of this finding are, of course, possible. It seems probable that the neurotic has, in general, a poorer power of conscious control of his own mental processes, so that he might be expected to suffer more occasions for self-annoyance on account of situations of the kind described in the third test. It could therefore be argued that the difference in scores on this test is due to a difference in familiarity with the items or alternatively to a relative failure on the part of normal men to recognize these items at all. Neither of these explanations is, however, considered adequate, since there seems no reason to suppose that mere familiarity with a mishap should make it seem more annoying, nor to doubt that most normal people have had some experience of the situations listed in the third test, which are of the kind described by Freud as the 'psychopathologies of everyday life'.

An alternative interpretation, of a highly speculative kind, may be offered in the light of Rosensweig's(4) suggested classification of aggressive reactions into the intropunitive and extrapunitive, whereby guilt, remorse and a need for punishment are contrasted with hatred, hostility and a need to punish, the essential distinction being in the direction of aggressive needs inwards or outwards. On this basis, the neurotic group would be described as abnormally intropunitive rather than extrapunitive and, on Rosensweig's suggestion, obsessional rather than paranoid in reaction. It will be noted that Rosensweig's third reaction, the 'impunitive' which he relates tentatively to 'hysteria', is not covered by this enquiry.

That the neurotic group should be relatively lacking in 'extrapunitive' tendencies is consistent with the fact that the predominant diagnosis in this group is 'anxiety neurosis', a condition shown by Slater(1) to be related to lack of overt aggressiveness. On the other hand, although obsessional characteristics are not conspicuous in the neurotics as a whole, so that no evidence can be adduced on this ground that they tend especially to guilt feelings and self-punishment, the opinion is widely held that these tendencies are characteristic of psychoneurotics generally, so that the interpretation is considered to have a fairly high probability *a priori*. Moreover, depression, in which these tendencies are also considered to be accentuated, is very conspicuous in the abnormal group.

It is thought, however, that no definite conclusion can be reached in this highly speculative field without the use of a considerable body of introspective evidence. This it has so far been impossible to collect, since the great majority of the subjects tested lacked the necessary skill. The limited evidence of this kind which is available does not, however, in any way refute the interpretation given above. A comment heard from some of the normal men in passing over the items in the third test was "that doesn't annoy me; it's my own fault" whereas the attitude of many of the neurotics seemed to contain a painful exasperation which may be summed up by the expression "I could have kicked myself". The suggestion tentatively put forward is therefore that to the normal soldier, the

recognition that a situation is his own fault seems to be somewhat incompatible with being annoyed about it, whereas to the neurotic it is a source of peculiar irritation.

To summarize, the three tests bearing upon personal annoyance are shown to discriminate between the two groups tested. Many interpretations of the findings are perhaps possible. That which is considered psychologically most probable is that the tests discriminate in so far as they measure, first a relatively greater sense of personal inadequacy and secondly a proneness for aggression to be directed against the self. The extent to which these tendencies overlap cannot be determined within the limits of this experiment, but both may be contrasted with self-assertive aggression directed towards the furthering of objective aims.

VI. DISCUSSION OF 'IMPERSONAL ANNOYANCES': COMPARISON BETWEEN TESTS 4 AND 5

Reverting to Table 1, it is seen that, although the two groups do not differ in their responses to test 4, the scores of the neurotics on test 5 are significantly greater than those of the normal men. The two sets of situations are, however, of very much the same kind, the only intended difference being in the sense department concerned. An exact comparison is afforded by the analysis given in Table 7.

Table 7. *Analysis of variance of scores on test 5 corrected for test 4*

	Sum squares	D.F.	Mean square	<i>t</i>	<i>p</i>
Within group	2232.320	247	9.038	7.77	0.001
Between groups	545.988	1	545.988	—	—
Total	2778.308	248			

The analysis shows that, when allowance is made for the association between the two tests, the discriminating power of the fifth is maintained and apparently enhanced. It, therefore, seems reasonable to conclude that the neurotics show a specific sensitivity to noise.

Several hypotheses may be put forward to account for this finding, but all are of a speculative kind. It may be argued that, since noise is one of the most primitive and potent fear stimuli, a heightened sensitivity to it is expected in those whose threshold for anxiety-producing stimuli is abnormally low. This supposition implies that the irritation experienced is related to a precipitation of anxiety. This view is consistent with the results of Harmon, Smith and Laird, and others on the psychological reactions of normal subjects to noise; results summarized by Bartlett(5) indicate a temporary autonomic response which is suggestive of a fear reaction. The limited introspections available from noise sensitive subjects within this experiment have not, however, confirmed an anxious element in the irritation experienced and further evidence on this point is sought.

Other explanations are suggested in terms of the distribution of attention. Bartlett(5), in summarizing the results of the Cambridge school on the distracting effects of noise on work, concludes that these effects are probably greatest first when the work puts a demand on the highest and most complex mental processes and secondly when attention is imperfectly absorbed in the task in hand; and that where these conditions obtain there is an increased difficulty in shutting out distracting stimuli of this peculiarly obtrusive kind. These conclusions suggest that the neurotics' hypersensitivity to noise may be partly due to a general failure to direct attention satisfactorily upon everyday objective activities. Inability to concentrate is a very common symptom among neurotics of the type tested and this inability could be attributed to a prior absorption of attention

by subjective preoccupations. These arguments do not apply to the visual irritants described in the fourth test, since the situations there described are such that they can be very easily overlooked unless there is a positive interest in the environment.

VII. CONSIDERATION OF THE TOTAL BATTERY

The discriminant functions for the various tests have been calculated by the method due to Fisher and described by Mather(3). The following weights are found to yield the maximum discrimination:

Test	Weight	Test	Weight
1	-5.6	4	-10.8
2	+2.0	5	+11.3
3	+8.0		

When scores on each test are multiplied by the weight for that test and summed, the mean weighted score for the neurotics is found to be +20.4 and for normal men -29.1 with the standard deviation of 44. The total mean score for the whole sample is thus -4.35. If any score greater than this is classified as 'neurotic' and any score less as 'normal', it is estimated that approximately 30% of the patients tested would be misclassified, since deviations greater than 24.75 from either group mean would occur in 60% of cases, half of which deviations would be in the wrong direction.

Since this test can be simply administered in about 5 min., it is thought that it might, in conjunction with other tests, be of some assistance in the large-scale selection of men liable to neurotic breakdown, at any rate under service conditions.

VIII. SUMMARY

A battery of five tests was administered to 250 hospital service patients, half of whom had no record of neurotic disorder. The tests consisted each of 12 sets of descriptions of possibly annoying situations of a defined type, and the subjects were asked to record which of these situations annoyed them. Comparisons between scores on the various tests were made and the battery was found to discriminate between neurotic and normal subjects. Attempts have been made to interpret these findings in the light of psychological content of the tests and to relate them to psychiatric theory.

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'REWARD' AND 'PUNISHMENT' IN LEARNING

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(1) *Thorndike's results* (pp. 83-84). (2) *Description of the present experiment* (p. 84). (3) *Results of experiment* (pp. 84-85). (4) *Introspective evidence* (pp. 85-86). (5) *Discussion and conclusions* (pp. 86-87).

(1) THORNDIKE'S RESULTS

Thorndike set out to prove that, other things being equal, one right response to a certain situation rewarded by the announcement of "right" strengthens the connexion in question more than one wrong response to the situation punished by the announcement of "wrong" weakens that connexion. This he showed, and other experimenters have agreed with his findings. He made, however, the further assertion that the announcement of "wrong" in his experiment did not weaken the connexion at all, so far as he could see. "Rather there is more gain in strength from the occurrence of the response than there is weakening by the attachment of 'wrong' to it. Whereas two occurrences of a right response followed by a 'right' strengthen the connexion much more than one does, two occurrences of a wrong followed by 'wrong' weaken that connexion less than one does" (*Human Learning*, p. 45. New York, Century, 1931).

Although it is probably true that most people learn more readily from right rather than from wrong responses, Thorndike's statement that "in general, annoyers do not act on learning by weakening whatever connexion they follow" is open to question. The experiment from which Thorndike drew his conclusion is described in *Human Learning*. Each subject had to choose one out of five possible meanings for a Spanish word by repeated attempts, each response being followed by the appropriate announcement of "right" or "wrong". There were 200 stimulus words, each being followed by its five possible English meanings, and these were studied by nine subjects. Thorndike took into account only those cases where the responses were either both wrong or both right in the 2nd and 3rd trials, or in the 3rd and 4th, or in the 2nd and 4th, in order to compare 'reward' and 'punishment'.

He noted twenty cases in which the right responses had been given in the 2nd and 3rd trials, and twenty cases in which the same wrong responses had been given in the 2nd and 3rd trials, and compared and tabulated in detail the results of the subsequent trials. It may be observed that only three of the same wrong responses out of the series of twenty had any great persistency; the other seventeen incorrect responses were avoided much sooner. These three series of wrong responses may have weighted his results in a misleading fashion.

For his total results he took:

The number of cases where right responses in the 2nd and 3rd or 3rd and 4th or 2nd and 4th trial, but not before, were followed in the next trial:

(a) by a right response	...	...	...	174
(b) by a wrong response	...	...	...	73
% which (a) is of (a) + (b)	...	...	...	70
% due to chance	...	...	...	20
Therefore strengthening	...	...	...	+50%

The number of cases where two wrong responses were followed by:

(c) any response other than it	...	...	373
(d) by the same wrong response	...	...	137
% which (c) is of (c)+(d)	...	...	73
% due to chance	...	...	80
Therefore weakening	...	...	-7%

He also listed the influence of "right" after one right response, and of "wrong" after one wrong response:

The number of cases in which a right response in the 2nd, 3rd, or 4th trial was followed by:

(a) a right response	...	...	283
(b) a wrong response	...	...	338
% which (a) is of (a)+(b)	...	...	46
% due to chance	...	...	20
Therefore strengthening	...	...	+26%

The number of cases in which a wrong response was followed by:

(c) any other response	...	...	1609
(d) same wrong response	...	...	488
% (c) is of (c)+(d)	...	...	77
% due to chance	...	...	80
Therefore weakening	...	...	-3%

(2) DESCRIPTION OF PRESENT EXPERIMENT

In an experiment which was devised to gain more information about the challenging conclusions reached by Thorndike, each subject was presented with a successively increasing number of 'stimulus' words, and to each 'stimulus' word there were six possible Spanish response words (i.e. *Stimulus word*: Fruit. *Possible Spanish response words*: Paraquas, Ciruela, Naranje, Bolsillo, Manzana, Jumento). Each 'stimulus' word or group of 'stimulus' words was presented twice with an interval of 3 min. filled in with conversation. Instructions given to the subjects were:

This is an experiment to measure the effects of "right" and "wrong" replies on learning. You will be given an English 'stimulus' word to which 6 possible Spanish words will be attached, 3 of which are right and 3 of which are wrong. You should underline 3 words in each set of 6, and you will be told which are right and which are wrong. You will later be given another trial, and on the second trial the words will be reshuffled, and as the experiment proceeds the number of stimulus words will be increased from 1 continuously until 5 and then to 10.

All the subjects naturally assumed that they were to try to repeat the responses which were right and to eliminate those which were wrong. They all gave their full co-operation. Ten subjects were used, six being women university students (group A) and four being members of the Working Women's Summer School (group B).

(3) RESULTS OF EXPERIMENT

From this experiment the following results shown in Table 1 were obtained.

It can be observed from the set of total results that with the increase of material the observed results gradually approach the results which might be expected if there was no learning and the results were a matter of pure chance. Although the ratios of the total observed 'same rights' to the expected 'same rights' and the observed 'same wrongs' to the expected 'same wrongs' differ from 1 by 0.6 and by 0.49 respectively, it should be noticed that with the increase of test material there is a greater fall off of learning

from the 'wrongs' than from the 'rights'. In the first two tests the improvement from the 'wrongs' is slightly greater than the learning from the 'rights'. In test VI, however, the ratio of the 'rights' differs from 1 by 0.53, whereas the ratio of 'wrongs' differs from 1 by 0.40. It is difficult to explain why in test V the improvement from the 'rights' falls off, while that from the 'wrongs' increases so markedly as compared with the general trend. It is just possible that the 'wrongs' were easier to remember because they appeared more distinctive or familiar to the subjects, but there seems to be no really satisfactory explanation in the test material.

It seems beyond question, however, that the subjects in this experiment have learnt to eliminate responses by being told that they were wrong. When the material was well

Table 1

Test no.	No. of words right in first trial	Expected same rights in second trial	Observed same rights in second trial	Ratio: Observed/Expected	No. of words wrong in first trial	Expected same wrongs in second trial	Observed same wrongs in second trial	Ratio: Observed/Expected
Total results								
I: 1 stimulus word	19	9.5	17	1.79	11	5.5	0	0.0
II: 2 stimulus words	34	17	30	1.76	26	13	3	0.23
III: 3 stimulus words	40	20	35	1.75	50	25	10	0.40
IV: 4 stimulus words	63	31.5	52	1.65	57	28.5	19	0.67
V: 5 stimulus words	70	35	51	1.46	80	40	18	0.45
VI: 10 stimulus words	146	73	112	1.53	154	77	46	0.60
Totals and ratios	372	186	297	1.60	378	189	96	0.51
Individual results								
Subject								
I	37	18.5	26	1.46	38	19	4	0.21
II	39	19.5	36	1.85	36	18	7	0.39
III	43	21.5	42	1.96	32	16	4	0.25
IV	36	18	34	1.90	39	19.5	5	0.25
V	38	19	32	1.68	37	18.5	7	0.38
VI	39	19.5	28	1.44	36	18	16	0.89
VII	31	15.5	16	1.03	44	22	8	0.36
VIII	37	18.5	24	1.35	38	19	25	1.32
IX	35	17.5	25	1.43	40	20	17	0.85
X	37	18.5	34	1.84	38	19	3	0.16
Group results: Group A	232	116	198	1.71	218	109	43	0.39
Group B	140	70	99	1.40	160	80	53	0.66
Totals and ratios	372	186	297	1.60	378	189	96	0.51

The expected same right or same wrong was obtained by halving the number of words right or wrong respectively in the first trial.

within their scope, they have learnt to eliminate responses that they were told were wrong almost to the same extent as they have learnt to repeat responses that they were told were right. Furthermore, it can be seen from the individual results that whereas some subjects only learnt, or learnt considerably better, from being told that a response was right (i.e. subjects II, VI, VIII, and IX), some learnt to eliminate responses almost to the same extent as they learnt to repeat them (i.e. subjects X, V), while others learnt better from their mistakes (i.e. subjects VII, I).

(4) INTROSPECTIVE EVIDENCE

In summarizing the subjects' introspections, it may be pointed out that: (a) On the whole the subjects reported that they found it easier to remember the 'rights' than the 'wrongs'. (b) The more successful subjects tended to form some sort of system, or find

reasons for remembering to repeat or avoid chosen words. (c) Almost all the subjects, particularly at the later stages, reported that they had mixed up the replies to a set of words or that they had remembered underlining a word but could not remember whether the reply to it had been 'right' or 'wrong'.

Subject II said that she worked out a system for 'rights' and a system for 'wrongs', but that she sometimes became confused between the two. Subject IV reported that she remembered by the look of the word. Some words appealed to her and she definitely remembered them as being right or wrong according to what she had been told. Sometimes, however, she felt put off for some reason, and then she knew that she "could not cope". She felt that to begin with she remembered the 'rights' and 'wrongs' about equally, but she became annoyed if she had a double wrong, though she was not worried by one wrong in a set of three. She tried to connect the words with French or Latin; or if she liked the look of the word she would remember it for itself. If a word was outstanding, it made an impression, but 'meaningless' words were difficult to remember. The further they seemed from English, the easier they were. She found that 2 'rights' and 1 'wrong' were more difficult to remember than 1 'right' and 2 'wrongs', since she felt that in the latter case she must not let herself down. If there were 3 'wrongs' she remembered the 'wrongs' for themselves. As can be seen from the individual results, this subject scored highly.

Another high scorer, subject III, said that she found "10 a squeeze on her mentality" (i.e. test VI), but that she found all the other tests all right. She remembered the 'rights' by remembering the reasons why she had thought they might be right before choosing them. She remembered the 'wrongs' because she remembered them as being wrong, or because they had just "gone out". If she had not built up any reason before making a choice and the choice was right, it was difficult to find a reason for it being right in order to remember it. Alternatively, if it was wrong she felt "a sort of pity" for the word, and just ignored it later. This subject seemed to make especially strenuous efforts to remember the 'rights'.

Most of the subjects tried to work out some system with initials of letters and so on, but generally did not get much help from them. Subject X, a most successful subject, after having underlined what she thought were likely looking words, then made up words or famous initials (such as B.B.C.) from the initials and then remembered which part of the made-up word was right or wrong. For example in test VI there was the word Carve followed by Trichear, Cortar, Agarrar, Tomar, Cincelar, Grabar. She underlined 'Cortar' which was counted right, 'Agarrar' which was right and 'Tomar' which was wrong. This spelt 'Cat', and she remembered that 'Tom Cat' was wrong. However in the next trial she underlined 'Grabar', 'Cincelar' and 'Trichear', having been confused by the similarity of the initials.

(5) DISCUSSION AND CONCLUSIONS

The results and introspections gained from this experiment seem, in general, to be opposed to the conclusions reached by Thorndike. In his experiment, however, the amount of material was much greater, and probably beyond the grasp of some of his subjects. In the devised experiment, especially in the VIth test, the subjects often reported that they remembered having made the response but could not remember the

experimenter's reply. It would seem that the subjects remember a response as being: (a) definitely right, (b) definitely wrong, or (c) as having been made, but the reply is not certain. In the case of (c) incomplete or half-learning has taken place. When the bulk of the material is considerably increased, the number of cases of incomplete learning is increased relatively very greatly. This incomplete learning is what probably occurred in a number of cases in Thorndike's experiment. Although the subjects might have remembered making the response, the reply attached to that response might take a number of repetitions before being fully grasped. The set-up of his experiment would favour his possibly erroneous conclusions, because the percentage chance deductions from the wrong replies is not comparable with the deduction from the right replies. For the subjects would still, in many instances of the repeated wrong replies, be in the state of half-learning, and would be repeating the response in order to clear up the doubt. Furthermore, it would seem to be much simpler to remember that one response is right and to learn to ignore the rest, than to remember which of the five were wrong while trying to find the right response.

In conclusion, it would seem justifiable to suggest that when the amount and type of material are well within the grasp of the subjects, they will learn from being told that a response is wrong to eliminate that response, almost to the same extent as they will learn to repeat a response by being told that it is right.

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PERSONALITY STRUCTURE AND MEASUREMENT

I. THE OPERATIONAL DETERMINATION OF TRAIT UNITIES

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1. *The neglected priority of morphological research* (pp. 88–91). 2. *The Achilles heel in factor analysis* (pp. 91–92). 3. *The varieties of covariation observations as seen by the covariation chart* (pp. 92–96). 4. *The covariation chart in relation to methodology* (pp. 96–98). 5. *The contribution to practical determination of personality traits* (pp. 98–99). 6. *'Real' functional unities and the notion of 'degrees of efficacy'* (pp. 100–102). 7. *Summary* (p. 102). *References* (pp. 102–103).

1. THE NEGLECTED PRIORITY OF MORPHOLOGICAL RESEARCH

Precise description and measurement of personality, upon which accurate developmental studies and, indeed, *all* general researches on personality ultimately depend, have reached a stage when it is imperative that psychologists reach agreement—and a correct agreement—regarding the meaning of a unitary trait and regarding the varieties of functional trait forms which exist in nature. A second unsolved problem in this field concerns the meaning and value of the distinctions made so readily in clinical and general psychology between what may be called different “modalities” of traits, i.e. between the classes represented by dynamic traits, temperamental traits and abilities. The present article will take up the problem of trait unities, while a future article will explore the formal definition and operational recognition of modalities. Neither functional unity nor modality has yet received a strict operational definition bringing it to a scientific level appropriate to the context of precise and practical mental measurement in which it is commonly found.

Although the importance of dealing with “functional unities” is constantly emphasized, alike in description, measurement or interpretation, no one appears to know, in any fundamental sense, what the criteria of functional unity are. Naturally the solution of this problem must precede attempts to define modality; for one cannot sort traits into categories before defining the entities to be classified.

The necessary ideal of all mental measurement is that each measurement shall represent some *single*, defined quality or aspect of mind, i.e. of abstracted behaviour. It must lie in a well-established continuum, along a dimension the direction of which is accurately known.¹ This requirement exists not only for measurements that have to do with the total personality—though strictly, every measurement in a psychological laboratory is a measure of personality—but it is in the realm of measurements of individuality as shown by test performances that the need has been most readily recognized. Whenever the psychologist describes a function with one noun, e.g. a mental set, reaction time, the super-ego, shyness, frustration tolerance, he implies that he deals with some kind of unity. Experimental psychology, despite its pretensions to some superior methodological standing, shares with the despised qualitative psychiatry the stigma of making unities by fiat. For it has generally not been content to consider its measurements as

¹ Among recent discussions which have clearly brought out the necessity for considering unitary traits that of Chambers⁽¹⁾ is particularly challenging.

physical, interactive⁽⁹⁾ measures, defined strictly by the physical situation, e.g. "a finger reaction to a note of pitch so-and-so", but has labelled them measures of some unitary function, e.g. "auditory reaction time", without first stopping to prove that all auditory reaction times correlate as part of a single unitary trait.

The present plea for making psychological research more scientific through attention to morphology becomes in the main an attempt to restrain longitudinal studies until cross-sectional research has been adequately prosecuted. The temptation in personality research, particularly in the clinical field, is to attempt to run before we can walk; in other words, to make developmental studies regarding the effects of heredity, training, etc., on certain traits without first having mapped out the true outlines of the trait in question. Before one can study change in an entity one must know what the entity is. These outlines are most readily mapped, in the first place, by cross-sectional investigation, i.e. correlation studies of various kinds of behaviour as they exist at a given age or time.

However, it is not entirely true to say that cross-sectional studies must *invariably* precede developmental studies⁽¹³⁾. As the following discussion shows, we have, in the past, falsely limited correlation procedure to certain traditionally studied relationships, and it is quite possible—though not in general so easy—to establish unitary traits by correlation observations of other kinds, including those on longitudinal data. This secondary possibility barely saves the narrow, laboratory experimentalist from falling into a methodological position which is entirely absurd. Blindly imitating the immature mechanical phases of earlier sciences he has confined himself to psychologically relatively peripheral issues, e.g. the special senses, because they can be easily controlled, instead of advancing to those more central cross-roads of many factors in personality which provoke the need for analytical, statistical treatment. Incidentally, this has resulted, especially in America, in the so-called 'experimental psychologists' and the so-called 'psychometrists' segregating themselves and their techniques in opposing camps, apparently for social and occupational reasons, to the detriment of the development of that underlying theory which proceeds on the same laws for both. It is one of the functions of this article to show that correlation studies in cross-sectional psychometric material, on the one hand, and that particular longitudinal form of study found in the laboratory, on the other, *can* be regarded in a new light as part of a single research approach to morphology.¹ Indeed it is only by the synchronized introduction of new techniques in these fields, enlightened by the perception of a common goal, that morphological questions can be solved at all.

In brief, the establishment of proved functional unities is not a need peculiar to personality study, but is really an entirely general and fundamental issue in psychology. It is, therefore, all the more unfortunate that effective advances in many fields have had to be postponed because the more superficial investigators have pursued the wild geese of colourful clinical and experimental hypotheses instead of combining to build these somewhat laborious foundation studies in morphology on which developmental research can alone go forward with confidence of accuracy.

¹ Cross-sectional and longitudinal correlation approaches can often be very profitably alternated in successive clarifications of a gradually emergent unity; but the very first step seems necessarily to be made by the cross-sectional approach. For example, a disease entity may become defined through a group of symptoms and through the typical course which it runs. But there has to be a nucleus of symptoms first recognized in order that a 'course' may be recognized.

Contingently we shall proceed on the common-sense conception of what constitutes a unity—physical or mental. The unity which brings together a set of lesser parts is recognized by the fact that its parts appear and disappear in company. It is recognized by their moving together, by their mutually influencing one another and by their responding in the same manner to applied influences. This reduces basically to *covariation* of manifestation, in a great variety of circumstances. In parenthesis one may point out that all unities are obviously relative, some breaking down more readily than others, all included in higher and including lower unities.

Before proceeding to the heart of the present issue it will aid discussion to summarize some clarifications regarding unities in personality which have been worked out in recent contributions. In the first place we have to distinguish two kinds of trait unity—that of *common traits* and that of *unique traits* (2). The former represent dimensions along which all people can be measured, but the latter refer to unitary traits peculiar to an individual, e.g. an esoteric interest, a unique symptom, a rare hereditary potentiality or defect.

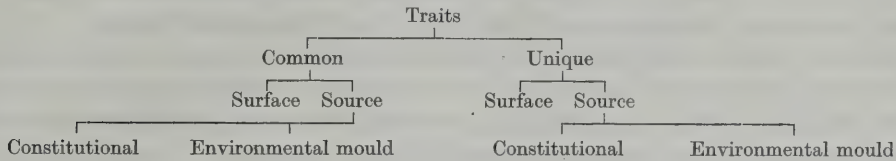
The covariation of behaviour elements, with respect to individual differences, or other differences, which leads to their being considered a common trait unity can be conceived, studied and operationally determined in two alternative mathematical or statistical ways. The correlation among the elements may, in the first place, be viewed as a correlation cluster. That is to say, the constituent elements have been picked out because, in a correlation matrix, every possible mutual intercorrelation among them reaches a value above some stipulated minimum. Such a cluster may be illustrated by Allport's 'Ascendance' (Dominance) pattern or by the syndrome of agitated melancholia, in which such trait elements as self-reproach, anxiety, crying, depression correlate positively and highly.

Alternatively the unity of covariation may be that of a mathematical factor arrived at by factor analysis. Here the elements share variation in a common dimension, but this dimension does not account for all their variation or even all their covariation. The present writer has suggested (3, 7) that the terms in clinical and general discussion which could most aptly be made to correspond to these two mathematical entities are *surface trait* (for the cluster) and *source trait* (for the factor). For the cluster is an empirical unity, observed more directly in behaviour structures,¹ and is as literal as the correlation coefficients themselves; whereas a factor is an interpretation—an attempt to explain, in terms of underlying causes or factors, the observed correlations. As instances of source traits one may consider Spearman's "*g*", Burt's factors of emotionality or Bleuler's and Kretschmer's "schizothyme tendency", in the last of which, for example, such trait elements as seclusiveness, fantasy production, hostility and rigidity are found to vary along the same dimension from real source influences.

On general grounds one may infer that source traits will be of two kinds—*constitutional* source traits and *environmental mould* source traits. For the influence which could account for a group of behaviour elements tending to vary together must be either a single hereditary focus—a gene—affecting all, or a single environmental source which constrains and affects simultaneously a certain pattern of habits. Conceivably source and surface traits can again be subdivided into dynamic, temperamental and ability

¹ Throughout, a 'structure' is considered a relatively fixed psycho-physical disposition—an abstraction from observed behaviour—which is held to be responsible for the intermittent reappearance of the actual behaviour.

modalities; but this will be discussed in a future article. Surface traits cannot be divided into constitutional and environmental mould traits, because each correlation cluster is likely to result from the overlap of two or more factors, i.e. it is thrown into relief by the coincidence of genetic and environmental factors. The relationships just described are summarized in Table 1.

Table 1. *Varieties of trait unities*

2. THE ACHILLES HEEL IN FACTOR ANALYSIS

It is conceivable that clusters and factors do not, or soon will not, exhaust for the mathematician the devices by which unitary covariation can be indicated. But that is unlikely; they seem adequate for all conceivable psychological purposes. For it can be seen that well-known traits, long described in verbal terms, do in fact appear to the statistician as clusters. Similarly, in trial situations of an experimental type where the real influences at work are known beforehand, or from ulterior considerations, mathematical analysis can 'discover' these same influences again as factors. It is also conceivable that some psychological reality might correspond to a hybrid between a cluster and a trait, namely a kind of covariation which is best described by using two axes at a moderate angular separation in the same dimension, such that they are separate but not independent influences. There is also the related possibility of considering as psychological entities what Thurstone has called 'second order factors' (15). However, there is as yet no evidence that all known psychological trait structures cannot be adequately represented by correlation clusters or by factors, allowed, when necessary, to depart from orthogonality.

Description of personality in terms of clusters is a desirable intermediate stage on the way to describing it in terms of source traits. That there should be some psychologists willing to stop at clusters, in spite of the representational defects which clusters show (7) as a device for final description, is due, as indicated above, to the failure of factor analysis to develop satisfactory methodological criteria for distinguishing between factors that are merely mathematical artifacts and those which correspond to real psychological traits. It is well known that factor analysis yields as many factors as there are *dimensions* required to represent the various directions of variation which can be graphically represented from the observed correlation coefficients. But though the *number* of factors can be sufficiently defined the particular set of factors or the particular *rotation* of factor axes which shall take shape in this multi-dimensional space cannot at present be determined by the given data and must be left to secondary considerations and arbitrary inclinations on the part of the experimenter. A given pattern of clusters—of surface traits—can therefore at present be accounted for by an immense variety of alternative factor sets—indeed an infinite variety within a certain series—only *one* of which can correspond to the real source traits functioning in that situation. It is not surprising,

therefore, that some psychologists have come to regard factors as mere devices for condensation of numerical data, despairing of equating them to real psychological influences.

So far the most promising answer to this indeterminacy has been Thurstone's principle of *simple structure*, which assumes that the most likely solution from the point of view of obtaining real functional entities is that which gives the most parsimonious explanation *with regard to the single matrix of correlations being studied*. Recently another way out of the factor analytic impasse has been suggested by the present writer—a way which, in so far as it depends on parsimony, interprets its role differently.

This new 'principle of parallel proportional profiles' (8) proceeds on the assumption that the true functional unities, and these only, can manifest themselves, as factors, simultaneously in different, independent, correlation matrices, arising from measurements of the same sets of variables under quite different conditions. According to this view any set of factors which can be extracted from one matrix only (even if simple structure be attained) is not proven to be anything more than a collection of mathematical artifacts, of predictive convenience only in the situation in which the correlations were made. Instead the mathematical solution of parallel proportional profiles aims at finding the positions of rotation in each of the independent factor matrices which will give recognizably the same factors for all matrices. The process of solution is akin to rotating the parallel cylinders of a combination lock to obtain the position in which all will yield the one release position. There is presumably an infinite series of positions of rotation for each matrix, but in only one instance can the factor loadings on one be matched by those obtainable from another. The practicability of a solution along these lines has been demonstrated by showing that such a position is unique and that it can be determined by the solution of simultaneous equations without the necessity for tedious trial and error rotations (8). It is too early to say how successful the method is with actual personality trait data.

It will be noticed that the above method requires as its raw material the provision of more than one correlation matrix from the same identical set of variables. Obviously this requires that the measurements for correlation be gathered under different conditions. What are these conditions? Matrices merely derived from two different random samples of the general population would not be independent. Within the limits of sampling error they would yield essentially the same correlation coefficients and therefore precisely the same series of possible factor sets. The principle of parallel proportional profiles can be employed only if the correlations, and therefore the factor matrices, are not identical. This is possible only *by allowing the same factors to be present in different strengths* in the two situations. The situations which would yield such related but different correlation matrices will now be explored systematically.

3. THE VARIETIES OF COVARIATION OBSERVATIONS AS SEEN BY THE COVARIATION CHART

Factor analytic and cluster studies aimed at discovering trait unities, as a glance at researches so far available will readily show, have implicitly assumed that correlation means the study of covariation among existing individual differences in a group, i.e. correlation of the departures of a number of individuals, at a given occasion of testing, from the mean, with respect to performances on a certain set of test variables.

The more novel contention of this article is that for the determination both of trait unities and also of trait modalities, covariation must be conceived and measured not only in this one 'dimension' but in every theoretically meaningful and practically possible sense. The senses in which covariation can possibly occur seem never to have been systematically explored. Omitting all the tentative groundwork which led naturally from accepted practices to the present formulation, we may present the finished theorem most succinctly by means of the diagram in Fig. 1, which we shall call a 'covariation chart'. This represents a system of rectangular co-ordinates in three dimensions, reproduced in perspective and arbitrarily truncated at some indifferent distance from the origin. The co-ordinates do not represent continua,¹ but, in each case, a population of points arranged serially, equidistantly but in no particular order. Along OA lies a series

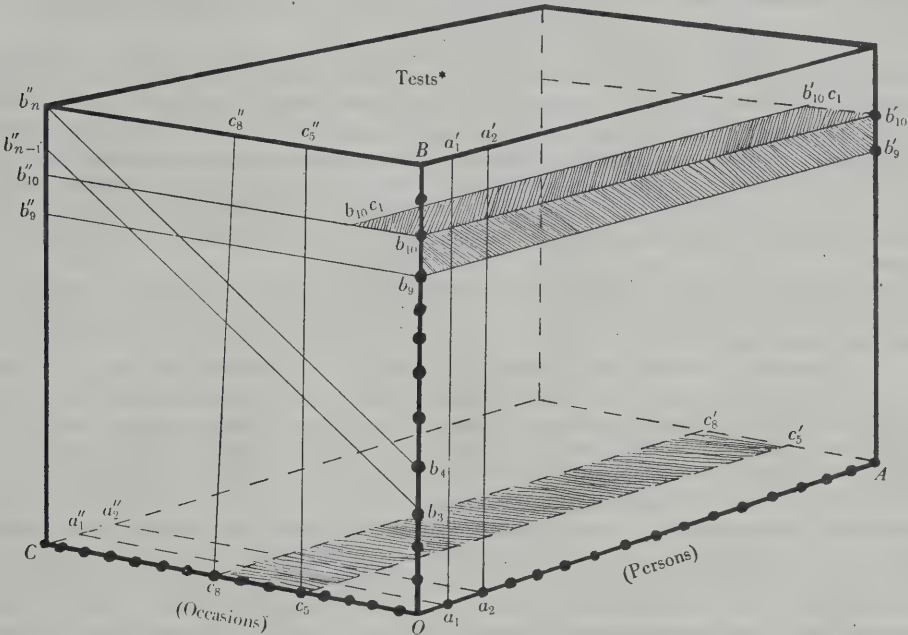


Fig. 1. The covariation chart

* The label 'Tests' is used for brevity and to fit the most frequent usage, but actually the vertical series is composed of 'any variables'.

of persons, a_1, a_2 , etc. Along OB lies a series of tests, b_1, b_2 , etc.; and along OC a series of occasions of testing, c_1, c_2 , etc.

Now a correlation coefficient, quantifying the extent of covariation, can be calculated between any two series of test measurements matched in some way item for item. Consequently a correlation coefficient is represented in the diagram by any pair of parallel (and in some cases non-parallel) lines of equal length (in terms of points). Apart from diagonal lines, which may momentarily be set aside as relating to comparatively rare, esoteric experimental designs, and non-parallel lines, which are largely psychologically meaningless, there are primarily six series which could be correlated, represented by the pairs of parallel lines one can draw rectangulary in each of the three bounding planes

¹ It is necessary to stress this, and to point out in particular that OC is not a time axis. The occasions of testing need not be separated by equal intervals of time or even be arranged in their true temporal sequence.

of the covariation chart (and in the many planes parallel thereto which can be drawn throughout the parallelopiped). These lines need not, of course, be immediately adjacent or begin at an edge or lie in a bounding face of the parallelopiped; but for ease of illustration we shall take as representative of each of the six possibilities a pair of parallel lines starting off from an edge and fairly close to each other. For the sake of later discussion each possibility of covariation study will be given a name, while the pairs of lines that can be drawn in the same plane will be classed together under a generic title, since they represent varieties of one basic form of covariation.

I. Six designs of covariation using (A) literal measurements

A.1. *Static covariation.* (Test-person variation. Occasion constant.) This is called static because it deals with measurements taken on only one occasion, as in a snapshot. The paired lines lie in the plane $OA-OB$ (and others parallel). The two possibilities are:

A.1 (a). *Static correlation and analysis of tests.* One correlates the scores of a series of persons on any two tests. This is the commonest of all correlation procedures and has been called *R*-technique (1, 14). It is illustrated by the lines $b_9b'_9$ with $b_{10}b'_{10}$, giving a paired series of persons' scores on the tests b_9 and b_{10} . (Also by any pairs of lines, adjacent or separated, parallel to these in this or parallel planes.)

A.1 (b). *Static correlation and analysis of persons.* One correlates a series of test scores with respect to two persons at a time. This has been called *Q*- or *P*-technique and has been employed principally where people are few and tests are many, to arrive at "types" and at the factors found more readily by *R*-technique when larger populations are available (1, 14). It is illustrated by the lines $a_1a'_1$ with $a_2a'_2$, etc.

A.2. *Monovariate covariation.* (Person-occasion variation. Test constant.) Here the variation is with respect to any one test and lies in the plane of one test only, i.e. in $OA-OC$ and planes parallel thereto. The two possibilities are:

A.2 (a). *Monovariate correlation and analysis of occasions.* One correlates the scores of a series of persons obtained on any two administrations of the same test, i.e. any single correlation has the typical form of a reliability or consistency coefficient. Illustrated by lines $c_5c'_5$ with, say $c_8c'_8$. Since the factor analysis of such correlations has never been attempted it may be permissible to digress into some examples of its usefulness. Obviously such analysis would reveal *groupings and functional unities among occasions*. If the test and its circumstances were kept entirely unchanged the unities would be states, e.g. appetite or maturational or physiological states in the organism (or in the social organism in the case of opinion measures, etc.). If, on the other hand, the test is kept constant but the influential circumstances are deliberately changed on the different occasions, the factors will also indicate the proper natural grouping of these circumstances. (This latter, modified situation, of course, could equally well be considered the application of actually different tests, as in A.1 (a).) This is the form of correlation that has been so unfortunately neglected by 'experimental' psychologists. It would throw light on the unity of learning phases, or on the factors in hearing or visual stimuli. In social psychology it could reveal similarities of phase in historical developments of group life. Its most valuable use, however, is in discovering the outlines of dynamic traits within the personality.

A.2 (b). *Monovariate correlation and analysis of person relationships.* One correlates any two individuals' scores on a series formed by many occasions of application of one kind of test. The correlated series are represented by lines $a_1a''_1$ with $a_2a''_2$. The factors from such correlations would be factors in persons, i.e. types, in relation to reaction to experimental influence, maturation, learning, fatigue, etc. With twins and variously related individuals this could be a method of study of hereditary tendencies. In social psychology, applied to such data as that of Whitehead on day to day output in the individuals of a group (16), it could reveal the patterns of social groupings.

A.3. *Intra-individual covariation.* (Test-occasion variation. Person constant.) Here differences between persons are not involved. The variation is that of time and test within a single person. The lines lie in the plane $OB-OC$ (and others parallel thereto). The two possibilities are:

A.3 (a). *Intra-individual correlation and analysis of test mutation relationships.* One correlates the scores of one person on two series formed by the repeated application of any two tests, as represented by the line system $b_9b''_9$ with $b_{10}b''_{10}$. A common factor would represent a unity among certain performances in relation to fluctuations produced by appetitive physiological, environmental, or other influences, or merely by time changes. This form of analysis is probably quite the most important of

those so far neglected in correlation studies. It is *the* method for discovering unique trait unities (2, 10) in the individual, especially those of a dynamic kind. It could be used to discover the syndrome form of a psychosis by longitudinal study, or the dynamic connexions in a neurosis. The present writer has begun a research employing tests of abilities, temperament traits and dynamic responses in a single correlation matrix from one individual to discover functional unities bridging diverse modalities.

A.3 (b). *Intra-individual correlation and analysis of occasions.* One correlates the scores (or rank orders) of a series of tests given to a person on one occasion with the scores on the same series of tests given on another occasion. The typical correlation is represented by the line system $c_5c''_5$ with $c_8c''_8$. The magnitude of the correlation represents the similarity of the person to himself, with respect to whatever range of personality manifestations are covered by the tests. Consequently a factor would represent a set of occasions upon which one type of personality in that individual tends to display itself. This is the mathematical device required for the objective investigation of multiple personality, especially with respect to the slighter changes between equilibrium positions existing in 'normal' people. By observing or controlling the circumstances of the occasions of measurement the factors discovered could be related to those discovered by other methods, e.g. A.3 (a).

These six designs necessarily exhaust the theoretical possibilities for manipulating absolute scores, i.e. if the correlated measures are to be literal measurements. But further designs, of great practical importance and clear psychological significance, emerge if one considers covariation in which the individual measurements employed are *differences* or *ratios* of absolute measurements. To be meaningful the differences or ratios have to be those between systematically paired scores. That is to say, they are differences represented by distances between paired lines in the diagram. Consequently the new designs obtainable can be exhaustively explored by substituting a *strip* for each line in the first set of designs, the strip being bounded by the two sets of scores between which differences are taken.

The strip, with respect to any one line, may lie in either of the two possible planes, i.e. either at right angles to the plane or in the plane in which the line lies. There are, it is true, other possibilities geometrically, but they are psychologically meaningless. One could, for instance, let the differences lie along the length of the edge in question, the scores used being the difference between each score and its immediate successor. For example, one could correlate a set of people with respect to their scores on two tests, the 'scores' being the difference of each from the score of his neighbour. It is just possible to imagine a bizarre situation in which this would be psychologically meaningful. Beyond these possibilities lie those in which one allows the two correlated strips to be in other than parallel planes. Confining the exploration to the more widely meaningful situations, one would find, by virtue of the two strips which are possible for each line, twelve possibilities in place of the six based on absolute measurements. They are outlined with great brevity below.

II. Twelve designs of covariation using (B) differential or ratio measurements

B.1. *Static covariation of differences.* This is analogous to A.1, having the edges of the strips in OA-OB.

B.1 (a). *Test increment correlation among tests.* One correlates increments in test scores, as between two occasions, for a series of persons on any two tests. A typical strip would be $b_{10}b'_{10}$, $b'_{10}c_1b_{10}c_1$ taken in conjunction with a parallel strip, e.g. one based on the edge $b_9b'_9$. For example, one might take many measures of hearing acuity, with respect to loudness, pitch, etc., repeat the measures after some years and attempt to separate the factors in hearing by factor analysing the correlations of changes. The factors will be developmental functional unities—of learning, maturation or physiological influence, according to the experimental design.

B.1 (b). *Test difference correlation among tests.* One correlates again two test 'scores' for a series of

persons, but each score is now the difference between test X and test Y in one series and test U and test W in the other. The strips will lie in the plane of $OA-OB$, one such being represented by $b_9b'_9$, $b'_{10}b_{10}$. One might, for example, correlate height-weight ratio with the difference between speed and fluency of association. The factors obtained should show relation to those from literal measurements, but the ratio method can bring them out in some cases far more clearly.

B.1 (c) and B.1 (d) are the Q -technique equivalents of 1 (a) and 1 (b).

B.2. *Monovariate covariation of differences.* Analogous to A.2, and in the same plane— OA, OC .

B.2 (a). *Monovariate correlation of occasions as occasion differences.* One correlates a series of persons on one test on two occasions, the 'score' on each occasion being actually the ratio of scores of some two occasions. One such series is represented by the strip $c_5c'_5, c'_8c_8$ in which the occasion is c_5 , but the score of each person is the difference between c_5 and c_8 . This might be correlated with the difference between, say c_{11} and c_{12} . It is impossible in this review to give proper credit to the possible meanings of such occasion factors.

B.2 (b). *Monovariate correlation of occasions using test differences.* One correlates two occasions, using a series of people, the scores being test score differences between two tests. A factor here would run through a set of occasions in which people's differences as between two performances tend to be alike. It is a likely design for exploring dynamic traits. One might, for example, correlate the difference between an intelligence and an attainment test on occasions near to and remote from an important examination period. Like their equivalent in literal measurements—A.2 (a)—these two designs, B.2 (a) and B.2 (b), could be especially useful in studying the dynamics of personality and the behaviour of social groups.

B.2 (c). *Monovariate correlation of persons with respect to person differences.* One correlates persons on a series of occasions, the score on each occasion being a difference between two persons. The question could be posed for example, "Is there any relation between the differences between A and his wife and those between B and his wife?" The device seems indicated principally for social psychology, the factors being common influences in social occasions upon certain sets of people.

B.2 (d). *Monovariate correlation of persons with respect to test differences.* One correlates two persons on a series of occasions, each 'score' being a difference between two tests. The factors will be those of common patterns of internal differences. They should relate to the longitudinal factors, e.g. dynamic factors, found on absolute, literal measures, in so far as Q -technique and difference factors relate to R -techniques and literal factors.

B.3. *Intra-individual covariation of differences.* This is analogous to A.3, and the edges of the strips concerned lie in the same plane, namely OB, OC .

B.3 (a). *Intra-individual correlation of tests with respect to test differences.* One correlates differences of tests, with respect to two tests, on a series of occasions. The factors will be, analogously to A.3, those of unique traits and, most readily, dynamic traits. For example, one might study the difference of two drives, by the preference or obstruction method, in one rat on several occasions, and correlate with some difference between two abilities, to see how they are functionally associated.

B.3 (b). *Intra-individual correlation of tests respecting person differences.* One correlates two tests on a series of occasions for one person, with respect to his difference on each occasion from some other person (or a different person for each test). This might be used to throw light, somewhat obliquely it must be admitted, on twin relationships and heredity.

B.3 (c). *Intra-individual correlation of occasions with respect to person differences.* One correlates two occasions on a series of tests, the measurements being differences between the given individual and some other or others. A factor would load most of the occasions on which the people concerned are most similar. This again seems a device principally for use in social psychology.

B.3 (d). *Intra-individual correlation of occasions respecting differences of occasions.* One correlates two occasions, using a series of tests, for one individual, the measures being differences between scores on these occasions and some others. This approach, analogous to A.3, could reveal equilibrium states in multiple personality.

4. THE COVARIATION CHART IN RELATION TO METHODOLOGY

While the above eighteen research designs do not utterly exhaust the possibilities of obtaining data for covariation analyses, they leave unlisted only designs of a second order of originality, dependent upon and derivable from the above. Consideration of those residual derivatives, obtainable by modification and recombination, will show,

however, that they are, theoretically, quite numerous. They are covered largely by four classes, as follows:

(1) Paired series obtainable by combining one of the first six literal measurement series with one of the difference or ratio series, e.g. correlating a series of reaction times with a series formed by the ratio of, say, inhibition to temperamental hyperthyroidism.

(2) In the ratio designs, the ratio, for each series, can be made either with different series or with respect to some one standard absolute series. For example, the difference as ratio of occasions could be with respect always to an occasion a day after the first, or all occasions could be compared with some one standard day.

(3) Various *oblique* parallel line pairs could be taken through the parallelopiped. For example, a series could be made from Smith's score on Monday, Jones's score on Tuesday, Roe's score on Wednesday, etc. With one possible exception mentioned in a future article⁽¹⁰⁾ these seem to be meaningless, applying to no imaginable psychological problem.

(4) Conceivably some fourth or higher dimension could be added to Fig. 1. It might be suggested, for instance, that a person or test could be modified systematically by a series of changes made continuously in one direction, e.g. a test might be given under increasingly speeded conditions, or to persons increasing in age or hours of work. Actually this involves a misconception of the original scheme. There can be no fourth aspect to tests, persons and occasions. Any circumstances affecting these can be handled by means of the 'occasions' axis, arranging the occasions in order and in a continuum with respect to the introduced dimension, e.g. age, which is specifically involved. One could, however, employ a fourth axis if one wished to represent *two* kinds of modification of circumstance occurring simultaneously. But this is a recondite and esoteric development into which there is no point in digressing here.

If we compare these basic designs for the operational identification of functional unities through covariation observations with the designs which have been stumbled upon as tools in various research projects, the narrow, inadequate formulation of these 'regional' approaches becomes sharply illuminated. One sees that historically the immediate needs of particular experiments have alone dictated the directions of development of correlation technique—and dictated very blindly. We have had a mole's eye view instead of a bird's eye view of the theoretical possibilities.

In particular the incontinent outgrowth of applied psychology in the field of psychometrics, with its flood of merely repetitious treatments of trait measurement problems, created such an obsession with the *OA*, *OB* plane of the covariation chart, as to lead to the obscuring of other possibilities. To account for so complete a sterilization of imagination we have to remember also the competitive punctilio and pedantry which took charge of this field, especially in America, averting the discourse of competitors from any larger universe of thought.

When other directions of correlation crept into use they appeared in isolated fields of study and were not envisaged as part of a single set of principles for arriving through integrated study at the same functional unities. Factor analysts, and those concerned with individual differences, confined themselves, as indicated, to the plane *OA*, *OB*. Indeed for a time they confined themselves to a single direction in this plane: the correlation of persons. Meanwhile, experimental psychologists (in the narrow and obsolete sense of those who work only with instruments in laboratories) have crept, with less vigour than the psychometrists, over parts of the plane *OA*, *OC*. A third independent

development is illustrated by the clinical psychologists, who have employed, with more freedom, though not with any accuracy comparable to that of the other two regions, some of the possibilities of covariation study demonstrable in the otherwise neglected plane *OB*, *OC*.

The differently trained workers in the diverse fields and schools of psychology have behaved as if they moved in remote, unconnected worlds and, if they took note of one another at all, paid attention only to the *differences* of their methodological approaches. The present theoretical formulation, on the other hand, attempts to bring out what they have in common. It aims to transcend the viewpoints rooted in custom and to clarify, in a single system, the gamut of possible covariation observations, used and unused, on which inferences concerning psychological entities can be based.

One may perceive, in the light of this formulation, that the *R*- and *Q*-techniques, on which all extant factor analyses of abilities, temperament, and personality variables in general, have been founded, represent no more than the visible part of an iceberg. The untouched volume of observations, it is true, may not be quite as great as this suggests; for some of the submerged possibilities apparently do not correspond to readily practicable or convenient experimental or measurement procedures. Furthermore, if we are considering procedures only from the standpoint of personality study, the number is somewhat reduced, for, as a perusal of the above examples will show, some designs are of most use in social psychology, i.e. the study of group phenomena. The majority of the new possibilities, however, are designs each of which is capable of pointing to some entirely original experimental approach to existing problems. Some hold the promise also of producing novel psychological concepts for the more economical formulation of theories. Indeed the parallelopiped of Fig. 1 is a research worker's treasure chest, replete with suggestions for effective new research approaches, the data available from any one of which would defy exhaustion by the most zealous investigator.

5. THE CONTRIBUTION TO PRACTICAL DETERMINATION OF PERSONALITY TRAITS

Granted sufficient ingenuity in setting up the kinds of experimental research design which would permit co-ordination of data from diverse approaches, it seems that almost all of the above eighteen approaches could be used to throw light on functional unities in the personality, and in the personality in relation to its society. But in the most direct sense, and in ways demonstrable within the compass of a presentation lacking space for elaborate discussion, the approaches which can be considered immediately practicable specifically for personality study are less numerous. The covariation designs which apply particularly to the delineation of trait structure in the individual, and which offer themselves as sources of supply for those independent correlation matrices needed to determine rotation of axes by means of the principle of parallel proportional profiles, are as follows:

- A.1 (a). Static correlation of tests.
- A.1 (b). Static correlation of persons.
- A.2 (b). Monovariate correlation of person relationships.
- A.3 (a). Intra-individual correlation of test mutation relationships.
- B.1 (a). Test increment correlation among tests.
- B.1 (c). Test increment correlation among persons.
- B.2 (b). Monovariate correlation of occasions using test differences.

Some of these—those which occur together within any one sub-section, e.g. A.1, A.2, A.3—do not offer new, independent designs for gathering data but only new ways for handling them. For instance, the same set of actual observations can be analysed factorially either by *R*- or by *Q*-technique. However, to provide the sufficiently independent correlation matrices necessary for the method of investigating personality advocated here, it is important that each matrix should arise from a separate set of experimental data. This leaves us still, however, with the full number of sources listed above, for the *R*- and *Q*-techniques can be applied to different data, especially gathered to suit the advantages of each.

It will be observed that any functional unity which appears simultaneously (as manifested by parallel proportional profiles of factor loadings) in the above set of matrices can claim to have shown its persistent nature, by repeated common variation of its elements, in (a) the individual differences occurring in a population, (b) growth curves and (c) day to day fluctuation within one individual. Further independent matrices, which would yield factors comparable only in some respects, e.g. only with respect to hereditary, constitutional unities,¹ are as follows:

B.2 (d). Monovariate correlation of persons with respect to test differences.

B.3 (b). Intra-individual correlation of tests respecting person differences.

The following may be expected to yield factors related in more special ways—mainly as ‘difference’ factors mathematically related to the corresponding ‘absolute’ factors—to the factors in the above sets of matrices:

B.1 (b). Test difference correlation among tests.

B.3 (a). Intra-individual correlation of tests respecting test differences.

The principal absentees from the above list of methods claiming immediate practical utility are those involving the correlation of occasions. Yet these too can be used if the occasions are associated with particular *circumstances* of measurement and specific provocations to dynamic and temperamental trait-element reactions, which will act as signatures permitting identification of the nature of the unity binding the occasions. If, for example, using method A.2 (a), and measuring reaction time, one obtains a factor running through all occasions on which electric shock and other physical punishment is threatened for slow reactions, and another through all occasions on which social reward of various kinds is offered for quick reactions, it is possible to infer a unitary trait of “fear of physical punishment” and one of “responsiveness to social rewards”.

However, the methods which suggest themselves as most immediately practicable and convenient for supplementing existing factor analytic data and aiding in its interpretation are A.3 (a) and B.1 (a). Unfortunately the changes which form the observed data in these methods are likely to be so small that they cannot be safely obtained from rating methods such as are now mostly employed in personality study. For the fluctuations in individuals are slight, unless, as rarely happens, one is able to extend experiments into a duration which will yield large growth changes for all. Consequently the most effective use of these supplementary methods waits on the development of precise objective tests, of personality reactions of a significant kind, such as have recently been suggested by the writer (5,6).

¹ For example, the designs B.1 (b) and B.3 (b) could be used with a series of twin pairs, correlating the differences of the twins with respect to certain tests. The trait unities which are products of environment will then appear as factors, whereas those which are constitutional will presumably not manifest themselves, in the differences of identical twins.

6. 'REAL' FUNCTIONAL UNITIES AND THE NOTION OF 'DEGREES OF EFFICACY'

Clinical and general psychologists, especially when irritated by the difficulties of statistics, have been known to say that the 'unitary traits' obtained by factor analysis are mere mathematical entities having no necessary relation to 'psychologically-real functional unities'.

On the other hand, the notion that one particular system of mathematical factors corresponds to a functional reality, whereas others do not (which has been propounded in § 2 above) appears to be doubted by some eminent factor analysts. Holzinger, for instance, points out that alternative factor resolutions *may* correspond to different aspects of some reality and that convenience alone can then decide which is to be used for some particular purpose. The present writer holds that first and second order factors constitute equally acceptable alternative aspects of a reality, but that apart from these the alternative factorizations cannot be considered equally real.

It behoves us therefore to face this question of 'reality', as it is bandied about between the clinicians and the mathematicians.

The common sense conception of a unity seems to be something the parts of which have similar properties as they affect the various senses;¹ but which, especially, move, *appear and disappear* together, which develop and decline together, and which have cohesiveness such that an influence on one part tends to be an influence on all.² A man who suspects that the elephant he sees in his garden is nothing but a juxtaposition of tree forms aided by the disturbance of his own vision, gets up and looks at it from a new angle to see if it still preserves its unity, or he waits to see if its parts move together and, if he is very bold, he may attempt to provoke it into moving as a unity!

The criteria of essential unity implicit in our correlational procedures are really no different from these. We seek elements that function together, as evidenced by comparison of functions in different individuals, that fluctuate together as shown by intra-individual studies, that grow and decline together in ageing, and which may be provoked to appear together by experimental influences. In the less systematic and exact clinical approaches we really do the same, though it is usual to start with some working idea of the unity and observe the symptoms in this 'syndrome', asking, in the trial and error of experience, "Is it what I think it is?" until the mental concept gets rough-hewn to the facts.

However, the covariational approach seems to introduce the initially alarming notion that there must be *degrees* of reality among unities. For on general psychological grounds it is probable—and in some known experimental instances certain—that there are functional unities which will manifest themselves as a unity of covariation only in *some* of the covariation research data and designs in which we look for them, whereas others will appear as factors wherever their elements are taken as variables into a correlation matrix.³

¹ That sensory similarity is less important than covariation of parts can be readily seen by reflecting on the example of a camouflaged gun.

² Philosophers sometimes distinguish between an ordinary unity and a unity which is a system. This last criterion—that the whole may be influenced powerfully through a part—seems to belong more to a system.

³ For example, it is pointed out by Lashley and by Hebb(12) that 'g' or general ability, which appears both as a factor in measurements of individual differences and in studies of growth increments, may yet not be a complete functional unity, in the sense of being a single mental energy or brain function, since some local injuries in adult brain do not affect at all some functions regarded as manifestations of 'g'.

To travel ahead instead of in the trail of past philosophical errors, the psychologist does well to ask what philosophy has made of this conception of reality. Plato and his earlier successors admitted grades of reality, but modern philosophy will hear only of the existent and the non-existent. For Plato a thing is real if it "exerts power"; for Aristotle the real is the "independent"; for Descartes it is that which has "perfection" or "completeness". All of these admit degrees.

However, since the only defensible meaning of real is 'existent', and since any factor, even that which has convenience only with respect to a set of variables as they appear in one particular correlation matrix, indubitably exists, we cannot accept, in the light of philosophy, that the clinician means what he thinks he means when he says factors are 'unreal'. But it is up to us to find out what he does mean, for like the respectable earlier philosophers listed above, he is trying to convey something which admits of degrees. Whether we say 'reality' or use some term to which modern philosophers are less allergic is irrelevant, but it seems necessary and very valuable to recognize that our discovered functional unities will lie in a hierarchy or continuum, from those which function in only one situation or type of situation, e.g. in growth, to those which have the highest degree of generality and universality, appearing in every correlation matrix, no matter what the conditions are under which the variables are observed.

Actually it would seem that into the determination of this hierarchy we must admit not only a count of those of the above eighteen covariation situations in which the unity shows itself, but also such considerations as the length of time the unity exists and the frequency with which it stands on its own or is subsumed as a 'part' in larger unities. To consider a simple physical example, the unity constituted by a railway engine is more 'real' than that of a wheel or a puff of smoke. It is more 'real' than the wheel because it is less often subsumed in larger wholes; it is more 'real' than the puff of smoke because it lasts longer. So the clinician means by a 'real' trait unity one which, in addition to showing itself in a satisfactory variety of contexts, cannot too readily be re-interpreted in terms of another whole (any second order factor⁽¹⁵⁾ which can be expressed equally well in terms of first order factors loses one degree of reality) and which lasts long enough to be of practical use in the prediction of behaviour.

Clearly these characteristics which in general give greater 'reality' to certain wholes resolve into a greater *usefulness* and *efficiency* in all kinds of explanatory and predictive situations. We shall therefore speak of 'degrees of efficacy' instead of 'degrees of reality' and open up the question of classifying covariational unities in a hierarchy (a hierarchy quite distinct from that of wholes-subsumed-within wholes) according to efficacy in general predictive situations.

A 'psychologically meaningless' mathematical factor, one of an infinite possible series from a given correlation matrix, having no claim to selection other than immediate convenience, and appearing only in this particular factorization, stands at the lowest level of efficacy, having a purely local usefulness. At the top of the hierarchy stand those traits which can impress themselves as factors on static, mutational, incremental and other covariation material, which are represented in physiological as well as psychological data, and which operate, perhaps as discernible second-order factors, even in sociological explanations. General mental capacity comes near to being at the ultimate peak of this hierarchy, as do the factors of manic-depressive tendency, general assertiveness and hyperthyroid tendency⁽⁴⁾. On the other hand, the factors of most unrotated,

unguided factorizations, such as are frequently employed to simplify the battery of ability measures in, say, a particular vocational guidance programme, though real and functional enough in the local setting, have the lowest degree of general efficacy. There is probably no such thing as a 'fundamental functional unity' in the sense of a unity which never breaks, just as, at the opposite extreme, there is no mere mathematical factor which is utterly unreal. One may hazard the guess, on general considerations, however, that the distribution of efficacy, utility or influential power of factors will be found to be bi-modal and such that most unitary traits will be either ephemeral mathematical conveniences or comparatively indestructible functional unities appearing in decidedly more than one plane of the covariation chart.

The task of psychology in setting up a framework of functions or variables for the exact description and measurement of personality should have as its goal the discovery of the most widely efficacious, influential unities, which offer the most economical set of factors for prediction when all the situations are considered in which psychology is called upon to predict.

7. SUMMARY

1. Trait unities are established by covariation of parts and can be classified first into *common* and *unique* traits and secondly into *surface* traits (correlation clusters) and *source* traits (factors).

2. In studying covariation by factor analysis the factors corresponding to psychologically meaningful functional unities can be distinguished from others by the method of parallel proportional profiles.

3. There are basically six and secondarily twelve sources of covariation data, expressible in a *covariation chart*, which can be used to provide independent correlation matrices for the above method.

4. It is meaningless to say that some factors represent *real* unities and that others are merely imaginary. All are real, but they may be arranged in a hierarchy according to 'degrees of efficacy'. The task of psychological comprehension and prediction demands the discovery of trait unities of a high degree of efficacy.

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LETTER TO THE EDITOR

THE ROYAL SOCIETY OF MEDICINE

THE SCIENTIFIC FILM ASSOCIATION

Preparation of a Catalogue of Medical Films, 1 Wimpole St, London, W. 1

Sir,

A catalogue of all the medical films in Great Britain is now being prepared by the Royal Society of Medicine in co-operation with the Scientific Film Association. It would be appreciated if any persons holding films of medical or para-medical interest, who have not already been asked for details, would communicate with the Film Cataloguer, Royal Society of Medicine, 1 Wimpole Street, W. 1. By so doing they would not commit themselves or their films, but would enable the catalogue to be complete.

Yours faithfully

GORDON GORDON-TAYLOR (*Surgeon Rear-Admiral*)
President, Royal Society of Medicine

ARTHUR ELTON
President, Scientific Film Association

PUBLICATIONS RECENTLY RECEIVED

David Eder: Memoirs of a Modern Pioneer. Edited by J. B. HOBMAN. Foreword by SIGMUND FREUD. Special Contributions by EDWARD GLOVER, LEONARD STEIN, HARRY ROBERTS and WYNDHAM DEEDES. London: Victor Gollancz, Ltd. 1945. Pp. 215. 8s. 6d.

The late Montague David Eder's interest in psychology developed early. Before qualifying in medicine he took a degree in psychology, of which he once wrote that "with that key, as a diligent student of Ward's enlightening article on psychology in the *Encyclopædia Britannica*, I was prepared to unlock any woman's heart". To one whose introduction to psychology was identical with Eder's, this confession of faith falls little short of the heroic. Yet it is wholly in keeping with Eder's life-long belief in a rational psychology and its ultimate capacity to interpret the deepest problems of human life and conduct. Not surprisingly, he later turned for inspiration to Freud and Jung, and is widely remembered as one of the wisest and most understanding of the earlier generation of psychoanalysts in this country. "M. D. Eder", wrote Freud in an all too brief preface to this book, "was one of those men distinguished by a rare combination of absolute love of truth and undaunted courage, together with toleration and a great capacity for love." Those of us who were privileged to know the late Dr Eder personally will not accuse Freud of indulging in conventional post-mortem eulogy.

This short volume of memoirs, cleverly edited by J. B. Hobman, presents Eder's life and work from a variety of angles. Yet the impression we gain, despite the multiple authorship, is one of consistency rather than diversity. Although Eder interested himself actively in a great number of fields it was never as a mere dilettante. His distinguished Zionist work in Palestine, for example, absorbed his full time and energies for several years, and it was only after a prolonged and distressing conflict of loyalties that he saw fit to return to London and medical psychology. In the same way, Eder as a social reformer could never be confused with that ubiquitous fraud of our day, the "parlour Bolshevik". A pioneer of what it is now fashionable to call social medicine, Eder was actively associated with the Margaret MacMillan Clinic at Deptford in the early years of the century, and he played, with Dr James Kerr, a notable part in the early development of the School Medical Service. He later made wise and stimulating contributions to child hygiene and education and, during the last years of his life, fought a brave battle in the campaign for proper employment of refugee medical men in this country. Right through his life, indeed, Eder effortlessly achieved that *rapprochement* between theory and practice so sternly demanded by the Marxists, and which, when consciously pursued by lesser men, so commonly issues in that bleak alliance of fanaticism and dogma typical of our day.

The principal interest of this book to psychologists will probably lie in Dr Edward Glover's contribution on "Eder as Psycho-Analyst" and Eder's own paper, "The Myth of Progress", delivered from the Chair to the Medical Section of the British Psychological Society in 1932 and here reprinted as an Appendix. Dr Glover gives us a shrewd analysis of the general development of psychoanalysis in this country and a just appraisal of Eder's not inconsiderable part in the movement. He traces in an interesting way the development of Eder's analytical thinking, though the somewhat brusque treatment accorded to his flirtation with Jung's ideas betrays a Calvinist rigidity of outlook that the subject of this memoir would surely have deplored. Dr Glover's concluding remarks on Eder's psychology will be of special interest to those who see in psychoanalysis an invigorating approach to biography.

Eder's paper on "The Myth of Progress" will be remembered as an honest and thoughtful discussion of a difficult subject and Mr Hobman is to be congratulated on seeing fit to include it in this volume. The paper cannot perhaps be called *original*, but then its author would have been the first to admit that his own mind lacked originality in the sense in which he recognized and admired it in so many of his friends and colleagues. Eder's unique distinction lay not in creative thought but in understanding, exposition and practice. And yet, despite his active bent and practical achievement, at heart he remained, as one of his earliest and closest friends might have put it, a Dreamer of the Ghetto.

General Psychology. By RAYMOND B. CATTELL. Cambridge, Mass.: Sci-Art Publishers. 1941. Pp. 624.

It cannot be denied that there is a great need of introductory and elementary text-books on psychology; and that in all probability the dearth of good ones is due to the difficulty of writing them. Prof. Cattell has made a gallant effort to supply this need, but like so many of his predecessors he has failed to achieve the varied aims of such books. Indeed, it may be questioned if it is possible to write a single book which is easy to understand, interesting to the beginner, coherently arranged, covers a wide ground and introduces a satisfactory amount of detail.

The early chapters of this book on Intelligence and Aptitudes, Motives and Character, do fulfil admirably the purpose of providing the student with an elementary and interesting introduction to psychology. Perhaps some brief consideration of social psychology, especially as regards the relationships of the individual to such characteristic groups as the family, school and neighbourhood, might also have been introduced here, since it is with the psychological problems presented by these relationships that the student is most familiar. Moreover, the discussion of social, educational and industrial psychology which is given in the last part of the book is on much the same straightforward and easily comprehensible level as that of the first chapters, and the two might have been combined. The student would find them more easy to tackle than chapters XI-XXI, which cover what is usually termed 'academic' psychology. Here also Prof. Cattell is much less happy in his treatment. In his desire not to leave out anything that might be relevant, he fails to make what is really important stand out from what is less essential such as detailed descriptions of physiological techniques and of the Mendelian theory of heredity, highly speculative theories of racial temperament, and relatively fruitless concepts such as that of perseveration. Further, his arrangement of his material does not seem either to place in the forefront what is most essential, or to lead the student logically from one topic to another. Why, for instance, describe the Nervous System in considerable detail, then Learning, Memory and the Thought Processes, and finally come back to Sensation and Perception? Lastly, it seems very doubtful if any treatment of statistics is not out of place in such a volume. It cannot be sufficiently detailed to be of much value, or simple enough to be readily understood.

The conclusion may be that it is clearer and less fatiguing for the student to be given at least two shorter books than one very long one. A first would consist of an easy and fairly popular study of psychological material of general interest; and a second of a more advanced treatment of 'academic' and experimental psychology. Such books require different methods of exposition; possibly it would be even more valuable to have them written by different authors from different points of view.

Psychology and the Social Pattern. By JULIAN BLACKBURN. London: Kegan Paul, Trübner, Trench and Co., Ltd. 1945. Pp. viii+157. 10s. 6d.

Dr Blackburn is to be congratulated on writing an excellent elementary text-book on general psychology. Since of recent years increasing emphasis has been laid on the social implications of psychological knowledge, it is important to effect a liaison between the discoveries of experimental psychology and the studies of sociologists, social historians, etc. Though frequent endeavours have already been made in this direction, they have seldom been made clear in introductory psychological text-books; and the student is too often left with the impression that the systematic study of psychology is something to be done in a laboratory, with little bearing on social conditions as he knows them. Dr Blackburn has kept this difficulty well in mind, and has written a book which will be found clear, readable and relevant by both psychological and sociological students. His treatment of intelligence and temperament testing and of the social influences which affect them is particularly valuable, not least as a corrective to the current vogue in these subjects. Occasionally he errs perhaps in going rather too far in the pursuit of readability. It is not quite just, for instance, to state the Freudian theory of repression in a form even more 'picturesque' than that of the original, and then pour scorn on its absurdities. Again, the amusing but bizarre nature of some of the rather numerous instances of psychotic behaviour renders them less suitable for the beginner to contrast with normal behaviour than are the characteristic features of neurotic behaviour.

It will be of great interest to follow Dr Blackburn's detailed discussion, in his forthcoming books, of social frameworks and relationships. The present volume breaks off rather abruptly leaving the study of motives and emotions in a somewhat sketchy state. Thus one hopes that this may be expanded and linked up with that of social problems which they influence to so marked an extent.

Intelligence Tests for Young Children. By C. W. VALENTINE. London: Methuen and Co., Ltd. 1945. Pp. xii+66. 4s. net.

The object of this book is to present for the use principally of teachers and those who are not expert intelligence testers, a series of individual intelligence tests for children from 1½ to 11 years. Eight to ten tests are given for half-yearly ages up to 4 years, and thereafter at yearly intervals, with rather fewer but longer tests for the older children. An excellent selection of both verbal and performance tests has been made; they are taken from Burt's revision of the Binet-Simon tests, the Terman-Merrill and Merrill-Palmer scales; some of the Gesell items are included; and the author has added some of his own. He also discusses the selection, use and standardization of the tests, and gives a very useful set of instructions and description of procedure. The whole forms an exceptionally valuable testing scale, consisting of items likely to be interesting and attractive to children and well designed to show up every facet of their intelligence. For certain of the biographical items relating to the younger children, which are taken from Gesell, the tester might have to rely on hearsay evidence; but these items are well worth including since they are such good indicators of the child's general maturity.

If one point of criticism may be offered, it is that the scoring of mental age is rather involved, owing to the fact that the number of tests, and hence their score value, differs at different ages. Perhaps in a second edition the author could simplify this—and also extend the scale for older children, including tests difficult enough for the controversial 11+ group.

Guidance and Personnel Services in Education. By ANNA Y. REED. Ithaca, New York: Cornell University Press (London: Humphrey Milford, Oxford University Press). 1944. Pp. xiii+496. \$4.75. 28s.

This book begins with a lengthy discussion of ways of collecting, recording, arranging, codifying, defining and categorizing all the available information about the methods used in the U.S.A. of applying what we call 'vocational guidance'. The discussion of testing, which follows in Part III, is too scrappy and contains too few concrete details to be of much value. Little effort is made to relate the individual abilities which can be deduced from test results to the abilities which can be demonstrated to be necessary by job analysis—as was done, for instance, in Oakley and Macrae's *Handbook of Vocational Guidance*. Part IV, on interviewing, is more interesting, but even in this the reader is overwhelmed by so much good advice of a general nature that he would have difficulty in knowing how to proceed on any specific occasion. Indeed, he may find it very hard to see the wood for the trees. Probably the bibliographies constitute the most useful parts of the book. But no references are given to any English work, except to Sir Francis Galton's use of free association.

Why Crime? Some Causes and Remedies from the Psychological Stand-Point. By CLAUD MULLINS. London: Methuen and Co., Ltd. 1945. Pp. 130+x. 6s. net.

A London policeman, with whom the reviewer happened to get into conversation the other day, remarked, "We policemen are too interested in running a fellow in, so that he may be out of the way for six months or so. We don't consider the fundamental reasons why he committed the crime, nor how the crime might be prevented." Perhaps if he reads this little book, he will agree that there are, as Mr Mullins suggests, four principal causes of crime—bad heredity, unwelcome births, parental discord and illegitimacy. To many there will be little that is new in this contention; but the author goes much deeper—into the causes of these causes—and shows how they might largely be eliminated. Among his proposals are that those who are unfit for life in the community at large should be detained for an indefinite period in suitable institutions; that the sizes of the families of some parents should be limited and of others increased; that in some cases voluntary sterilization should be encouraged; and that the law, particularly as regards divorce, should be reformed. His sympathy and concern are with the children, potentially good citizens or criminals, whose welfare is so seldom considered till most of the damage is done.

This is a most readable, but not superficial, account of what might be done with a view to the eradication of crime. It should be read very widely, and especially by those who have to do with the making of laws or have any interest in crime and its prevention.

H. B.

An Introduction to Physical Methods of Treatment in Psychiatry. By WILLIAM SARGANT and ELIOT SLATER. Edinburgh: Livingstone, Ltd. 1944. Pp. xii+171. 8s. 6d. net.

This is a very interesting and valuable account of many of the more recent physical methods of treatment in psychiatry—prefrontal leucotomy, malaria, soporifics, convulsants, etc. There is now no reason to despair of recovery, more or less complete, in cases of melancholia and schizophrenia, which often yield to insulin or convulsion therapy and are so resistant to psychotherapy; while malarial treatment has changed the outlook for the general paralytic.

Much of the work is purely empirical, but that does not detract from its value therapeutically. When the theoretical implications are understood, wider fields, both for psychology and psychiatry, may be disclosed. All who are interested in psychiatry, and particularly those who have to deal with patients in mental hospitals, will find this small volume of great service.

The Lady of the Hare: A Study in the Healing Power of Dreams. By JOHN LAYARD. London: Faber and Faber, Ltd. 1944. Pp. 277. 12s. 6d. net.

In 1940 the vicar of a small parish asked Dr Layard if anything could be done for Margaret, a girl of 16, who was extremely backward. She was later classed as a high-grade mental defective. Dr Layard saw the girl but could get nothing out of her beyond the words "Yes" and "No", and, when asked what books she liked to read, a whispered reply, "Edgar Wallace". So he turned his attention to her mother, Mrs Wright, who told him of her elder sister, Bertha, a disappointed woman who had never married, who had had a large share in Margaret's upbringing. Mrs Wright was an intelligent normal woman, the wife of a farm labourer, who, however, admitted she had found life difficult largely, it would seem, because she had to house and look after her very neurotic sister. And so the 'analysis' of Mrs Wright, which took place in twelve interviews, began. There were in all twenty-five dreams and visions the meanings of which Dr Layard explained to Mrs Wright. One of the dreams was of a live hare in a white bowl which she had to kill—a willing sacrifice.

Three years later, after some further interviews which are to be described in later volumes, Mrs Wright was found to be happier and more able to face her difficulties which had largely been resolved; her sister was more contented and gave less trouble, and Margaret was much improved. The latter was bright and happy and was found to be endowed with 'second sight'. She often knew what her mother had dreamed before she was told and had acquired a nascent power of spiritual healing. She was also having daylight visions and had knowledge of aspects of her mother's past history obtained through auditory and visionary contact with her maternal grandfather who had died when she was three. These visions of the grandfather developed, and eventually she was seeing "the Heavenly Father under whose direct guidance she now believes herself to be".

In Part II of the book the author gives at some length the result of his researches into the mythology of the hare, which he claims to be an archetype. In Part III more dreams about hares and rabbits are described.

The author has been encouraged and supported in his work by the members of the Psychological Study Group in Oxford, whose foundation members range from two professors (of Logic and of the Philosophy of the Christian Religion) to an anthropologist and a psychiatrist. Dr H. G. Baynes read the manuscript at least in part; thus presumably the analysis was approved by some Jungian psychologists.

To some psychologists, the fact that Margaret is seeing the Heavenly Father in visions would be a matter for concern, not for congratulation, for many such visionaries are to be found in mental institutions; while the help the mother received from the 'analysis' would probably have been equally well afforded by any sympathetic and understanding confidant without any reference to her dreams.

The study of the mythology of the hare may be of interest to anthropologists.

H. B.

Thematic Apperception Test. By HENRY A. MURRAY. Cambridge, Mass.: Harvard University Press (London: Humphrey Milford, Oxford University Press). 1944. 28s. net.

This revised form of Murray's *Thematic Apperception Test* consists of 30 pictures, and a manual of instructions and procedure. A brief description of methods of analysing the results is also given. The test is now suitable for use with men, women or children.

